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XVI 3

EXPERIMENTS AND OBSERVATIONS
ON
ANIMAL HEAT,
AND THE
INFLAMMATION
OF
COMBUSTIBLE BODIES;
BEING
An Attempt to Reſolve theſe PHENOMENA into a
GENERAL LAW of NATURE.

BY A. CRAWFORD, M.D. F.R.S. L. AND E.
AND MEMBER OF THE PHILOSOPHICAL SOCIETIES OF
DUBLIN AND PHILADELPHIA.

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WITH VERY LARGE ADDITIONS.

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MEDICAL SOCIETY OF LONDON

TO

RICHARD KIRWAN, Esq.

THESE

EXPERIMENTS AND OBSERVATIONS

ARE INSCRIBED,

AS A MARK OF RESPECT AND

ESTEEM,

BY HIS MOST SINCERE FRIEND,

AND

OBLIGED HUMBLE SERVANT,

THE AUTHOR.

ADVERTISEMENT.

THE Experiments which gave riſe to the following explanation of Animal Heat and Combuſtion, were made at Glaſgow in the ſummer of the year 1777. They were communicated, in the autumn of that year, to Dr. Reid, Mr. Wilſon, and Dr. Irvine. In the beginning of the enſuing ſeſſion, they were made known to many of the Profeſſors and Students in the Univerſity of Edinburgh; and in the courſe of the winter, they were explained by the Author, in the Royal Medical Society.

A brief account of theſe Experiments was laid before the public in the firſt edition of this work, which appeared in the year 1779.

As the trials recited in that publication had been made under many diſadvantages, I ſoon afterwards found, upon a careful repetition of them, that I had fallen into conſiderable miſtakes in my concluſions reſpecting the quantities of heat contained in the permanently elaſtic fluids.

I perceived, however, that theſe miſtakes did not affect the explanation which I had given of Animal Heat and Combuſtion. For though it appeared that the exceſs of the capacity of dephlogiſticated, above that of fixed air, was not ſo great as I had at firſt imagined; yet, from an extenſive ſeries of experiments, which were made with the moſt ſcrupulous attention to accuracy, it was evident that the difference of the capacities of thoſe fluids was ſuch as to afford an adequate explanation of the phenomena.

I have hitherto been induced to defer the publication of the reſult of my enquiry, that I might have an opportunity of giving the ſubject a more thorough inveſtigation, and of eſtabliſhing the leading facts upon as ſolid a foundation as poſſible. I was ſenſible that to correct errors, is the beſt method of apologiſing for them; and that though the free communication of diſcoveries is eſſential to the progreſs of knowledge, yet it is of much greater importance to the intereſts of ſcience, that facts ſhould be well aſcertained, than that they ſhould be ſpeedily publiſhed.

Many other cauſes have concurred to retard the publication of this Work. I might here mention the interruptions which have ariſen from want of health, from the avocations of a profeſſion that engaged a great ſhare of my attention, and from the difficulty of procuring the apparatus requiſite for nice and delicate experiments. I am perſuaded, however, that it is unneceſſary to employ much time in apologiſing for a delay, which the greater part of my Readers will probably rather approve than condemn.

A general conviction now prevails, that it is by the method of experiment and induction alone, that we are permitted to interpret the characters which the Deity has impreſſed upon his works; and thoſe who are in the habit of cultivating natural knowledge, well know that this method of ſearching into truth, neceſſarily requires much labour and patient inveſtigation.

Though I have uſed my utmoſt endeavours to eſtabliſh the facts, which I now venture to ſubmit to the public eye, upon a ſolid foundation; yet I am far from meaning to inſinuate that they are free from error. In an attempt to determine the relation which takes place between ſuch ſubtile principles as air and fire, we can only hope for an approximation to the truth.

I believe, however, that no conſiderable miſtakes will now be found, in the experiments recited in the following pages; and it is hoped that the candour of the public will aſcribe the errors that may ſtill remain, to thoſe imperfections in the ſenſes and in thermometers, which make it difficult to eſtimate, with preciſion, minute differences, and which muſt ever prevent us from arriving at perfect accuracy.

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ERRATA.

Page	Line
16	7, *for* places, *read* place.
31	5, *for* by 10lb. *read* to 20lb.—*for* .8 *read* 6.
	6, *for* 100.9, *read* 101.3.
53	16, *for* 5, *read* V.
71	7, *for* 6, *read* VI.
72	2, at the bottom, *dele* that.
85	12, *for* V. *read* VII.
134	1, *for* was, *read* is.
233	16, *for* +, *read* ×, *for* ×, *read* +, and *for* +, *read* ×.
256	5, *after* abſolute heat, *inſert* in equal weights.
270	12, *for* 1.57, *read* 1.55.
304	8, *for* 5, *read* 4.
	20, *for* 5, *read* 4.
316	1, *for* e f g h, *read* e f g n.

EXPERIMENTS

AND

OBSERVATIONS

UPON

ANIMAL HEAT, &c.

THE words *heat* and *fire* are ambiguous. Heat, in common language, has a double ſignification. It is uſed indiſcriminately to expreſs a ſenſation of the mind, and an unknown principle, whether we call it a *quality* or a *ſubſtance*, which is the exciting cauſe of that ſenſation *.

Philoſophers, who have applied themſelves to this ſubject in modern times, have generally appropriated the word heat to the unknown principle; and in the language of ſcience, its ſignification, as applied to that principle, is much more extenſive than in common language. For, in common language, it expreſſes ſuch a degree of the unknown external cauſe, as will produce a given effect upon the ſenſes; but in the philoſophical acceptation, it has been uſed to expreſs the external cauſe in the abſtract, without regard to the peculiar effects which it may produce. For the ſake of greater preciſion, in the following treatiſe, I ſhall, with the ingenious Dr. Irvine of Glaſgow, call the latter *abſolute heat*, and the external cauſe conſidered as having a relation to the effects which it produces, I ſhall call *relative heat*.

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* See Dr. Reid's Inquiry into the Human Mind, p. 78.

From this view of the matter, it appears, that *abſolute* heat expreſſes, in the abſtract, that *power* or *element*, which, when it is preſent to a *certain* degree, excites in all animals the ſenſation of heat; and *relative* heat expreſſes the ſame power, conſidered as having a relation to the effects by which it is known and meaſured.

The effects by which heat is known and meaſured are three, and therefore relative heat may admit of three ſubdiviſions.

1ſt, This principle is known by the peculiar ſenſations which it excites in animals.—Conſidered as exciting thoſe ſenſations, it is called ſenſible heat.

2dly, It is known by the effect which it produces upon an inſtrument that has been employed to meaſure it, termed a thermometer. This is called the temperature of heat in bodies.

3dly, It has been found by experiment that in bodies of different kinds, the quantities of abſolute heat may be unequal, though the temperatures and weights be the ſame. Thus it will appear from the experiments recited in the following pages, that if a pound of water and a pound of diaphoretic antimony have a common temperature, the quantity of abſolute heat contained in the former, is nearly four times that contained in the latter. When the principle of heat is conſidered relatively to the whole quantity of it contained in bodies that are of different kinds, but which have equal weights and temperatures, I ſhall term it comparative heat. If, for example, the temperatures and weights being the ſame, the whole quantity of heat in water be four times as great as that in antimony, the comparative heats of theſe ſubſtances are ſaid to be to each other as four to one.

Senſible heat is meaſured by the intenſeneſs of the ſenſation which it excites. Temperature, by the expanſion of the fluid in the thermometer: and comparative heat is meaſured by the alterations of temperature, which equal quantities of abſolute heat produce in bodies that have equal weights.

Thus we ſay that two bodies have the ſame ſenſible heat, when the organ of feeling is equally affected by them; and that the ſame body has a greater or leſs degree of ſenſible heat, according as it produces a greater or leſs effect upon that organ.

In like manner bodies are ſaid to have the ſame temperature, when they produce equal expanſions in the thermometer, and the ſame body is ſaid to have a higher or lower temperature, according as a greater or leſs expanſion is produced by it in that inſtrument.

It will hereafter appear, that the comparative heats of bodies which have equal weights and temperatures, are greater or leſs, in proportion as leſs or greater alterations are produced in their temperatures by equal quantities

tities of abſolute heat.—Thus it is found, that the ſame quantity of heat which raiſes a pound of water one degree, is ſufficient to raiſe a pound of mercury 28 degrees. From which it follows, as will be hereafter proved, that the comparative heat of water is to that of mercury as 28 to one—and conſequently, the alterations which are produced in the temperatures of bodies, by given quantities of abſolute heat, may properly be applied as a meaſure of their comparative heats; the alterations of temperature and the comparative heats being reciprocally proportional to each other.

Senſible heat depends partly upon the temperature,† and partly upon the ſtate of the organ of feeling; and, therefore, if a variation† be produced in the latter, the ſenſible heat will be different, though the temperature continue the ſame. Thus water at the temperature of 62 degrees of Farhenheit, appears cold to a warm hand immerſed in it; but on the contrary, that fluid will appear warm, if a hand be applied to it which has a lower degree of heat than 62. For this reaſon the thermometer is a much more accurate meaſure of heat than the ſenſes of animals. As long however as the organs

† [illegible]

† takes place in the substance or

but mercury will appear still colder although of the same temperature

that is supposing
our sensations
much more accu-
rate than what
we have reason
to suppose

remain unchanged, the ſenſible heat is in proportion to the temperature, And, therefore, theſe terms have generally been conſidered as nearly ſynonymous. It has been obſerved, by my learned friend Dr. Reid, in a very accurate communication, with which he favoured me on this ſubject, that "until the ratio between one temperature and another be aſcertained by experiment and induction, we ought to conſider temperature as a meaſure which admits of degrees, but not of ratios: and conſequently ought not to conclude, that the temperature of one body is double or triple to that of another, unleſs the ratio of different temperatures were determined. Nor ought we to uſe the expreſſions of a double or triple temperature, theſe being expreſſions which can convey no diſtinct meaning, until the ratio of different temperatures be determined." By not attending to theſe circumſtances, philoſophers have been led into miſtakes, in their calculations reſpecting the heat of comets at certain diſtances from the ſun.

When we examine the comparative quantities of heat in different bodies, we may conſider them either as having equal *weights* and temperatures, or equal *magnitudes* and temperatures; and the reſults of theſe compariſons will be different, in proportion to the differences of the ſpecific gravities of the ſubſtances compared. Thus it will be found, that the comparative heat of water, is to that of an equal volume of mercury, nearly as two to one; but in equal weights of theſe ſubſtances, the comparative heats are nearly as 28 to one. In the following treatiſe the comparative heats have been calculated from equal weights of the ſubſtances compared; this method appearing to be leſs liable to objection than the calculations from equal magnitudes, becauſe the latter requires a more perfect knowledge of ſpecific gravities than has hitherto been attained.

It is proper to add, that as equal weights of heterogeneous ſubſtances are found to contain unequal quantities of abſolute heat, there muſt be certain eſſential differences in the nature of bodies; in conſequence of which, *ſome* have the power of collecting and retaining that element in greater quantity than *others*. Theſe different powers are called in the following pages the capacities of bodies for containing heat: thus if we

~~[illegible]~~

~~[illegible]~~

The whole of this [illegible] [illegible]

[illegible] the word

we find, by experiment, that a pound of water contains four times as much abſolute heat as a pound of diaphoretic antimony when at the ſame temperature, the capacity of water for containing heat is ſaid to be to that of antimony as four to one.

The temperature, the capacity for containing heat, and the abſolute heat contained, may be diſtinguiſhed from each other in the following manner.

The capacity for containing heat, and the abſolute heat contained, are diſtinguiſhed as a force from the ſubject upon which it operates. When we ſpeak of the capacity, we mean a power inherent in the heated body; when we ſpeak of the abſolute heat, we mean an unknown principle which is retained in the body by the operation of this power; and when we ſpeak of the temperature, we conſider the unknown principle as producing certain effects upon the thermometer.

why not use the
term power alone

a thing unknown
when you talk
of something
absolute and
call it heat

The capacity for containing heat may continue unchanged, while the abſolute heat is varied without end. If a pound of ice, for example, be ſuppoſed to retain its ſolid form, the quantity of its abſolute heat will be altered by every increaſe or diminution of its ſenſible heat: but it will hereafter be proved, that as long as its form continues the ſame, its capacity for receiving heat is not affected by an alteration of temperature, and would remain unchanged, though the body were wholly deprived of its heat.

It was before obſerved, that the changes which are produced in the temperatures of different bodies, by equal quantities of abſolute heat, are *greater* or *leſs* according as the body to which the heat is applied has a *leſs* or a *greater* capacity for containing heat. The body, therefore, which has the leſs capacity for containing heat, has its temperature more augmented by the addition of a given quantity of abſolute heat, than that which has the greater. Hence the temperature of a body depends partly upon the quantity of its abſolute heat, and partly upon the nature of the body in which this heat is contained; and, conſequently, the temperature may be varied either by a change in the nature of the body itſelf, or by a change in the quantity of its abſolute heat. If the variation of temperature ariſes from the firſt of theſe circumſtances, it follows, that in the ſame body the temperature may vary, though the abſolute heat continues the ſame. If, for example, a body of a given weight be ſuppoſed to have a capacity as one, a quantity of abſolute heat as 10, and a temperature which computed from the point of total privation is alſo as 10; and if the capacity of the body be conceived to be ſuddenly doubled, the ſame quantity of abſolute heat which formerly raiſed it to the temperature of 10, will now be ſufficient to raiſe it only to the temperature of five. This will afterwards more clearly appear, from the experiments and obſervations on fixed and atmoſpherical air, venous and arterial blood *.

The following proportions obtain, with reſpect to the capacities, the temperatures, and the quantities of abſolute heat.

The capacities of bodies for receiving heat, are conſidered as proportional to the quantities of abſolute heat which they contain, when the quantities of matter are equal, and the temperatures are the ſame. Calling therefore the temperature T, the capacity

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* It is proper to obſerve, that the general fact reſpecting the influence of a change of capacity upon the temperature of bodies, was firſt diſcovered by Dr. Irvine.

pacity C, and the abſolute heat A, if the quantities of matter and the temperatures be given, the capacities will be as the abſolute heats—or if T be given, A will be as C. Again, in the ſame body, if the capacity be given, the quantity of abſolute heat will be in proportion to the temperature, computed from the point of total privation, and meaſured by a thermometer, the expanſions of which are proportional to the increments of heat. That is, if C be given, A will be as T.—Since, therefore, if T be given, A will be as C, and if C be given, A will be as T; it follows, that if neither be given, A will be as $T \times C$. Therefore C will be be as $\frac{A}{T}$, and if A be given, C will be reciprocally as T. Conſequently, if the capacities be reciprocally as the temperatures, the quantities of abſolute heat will be equal. Thus if the capacity of the calx of antimony be to that of the regulus, as three to one, and if the temperature of the former, computed from the point of total privation, be to that of the latter as one to three, the calx and regulus will contain equal quantities of abſolute heat.

Fire, in the vulgar acceptation of the word, expreſſes a certain degree of heat accompanied with light; and is particularly applied to that heat and light which are produced by the inflammation of combuſtible bodies. But as heat, when accumulated in a ſufficient quantity, is conſtantly accompanied with light; or, in other words, as fire is always produced by the increaſe of heat, philoſophers have generally conſidered theſe phænomena as proceeding from the ſame cauſe *; and have therefore uſed the word *fire* to expreſs that unknown principle, which, when it is preſent to a certain degree, excites the ſenſation of heat alone; but, when accumulated to a greater degree, renders itſelf obvious both to the ſight and touch, or produces heat accompanied with light.

In this ſenſe the element of fire ſignifies the ſame thing with abſolute heat.

Theſe definitions and remarks being premiſed, I ſhall proceed to give the reader a conciſe view of the general facts upon which the experiments recited in the following pages are founded.

---

* Vide Boerhaave, Martine, M'Quer, &c.

I. Heat has a conſtant tendency to diffuſe itſelf over all bodies, till they are brought to the ſame temperature. Thus it is found by the thermometer, that if two bodies, of different temperatures, are mixed together or placed contiguous, the heat paſſes from one to the other till their temperatures become equal; and that all inanimate bodies, when heated and placed in a cold medium, continually loſe heat, till, in proceſs of time, they are brought to the ſtate of the ſurrounding medium.

From this property of heat it follows, that the various claſſes of bodies throughout the earth, if they were not acted upon by external cauſes, would at length arrive at a common temperature, when the heat would become quieſcent: in like manner, as the waters of the ocean, if not prevented by the winds, and by the attractions of the ſun and moon, would come to an equilibrium, and would remain in a ſtate of reſt. But as cauſes continually occur in nature to diſturb the balance of heat, as well as that of the waters of the ocean, thoſe elements are kept in a conſtant fluctuation.

II. Heat is contained in conſiderable quantities in all bodies, when at the common temperature of the atmoſphere.

From the intereſting experiments which were made on cold, by my ingenious friend Mr. Wilſon, we learn, that at Glaſgow in the winter of the year 1780, the thermometer on the ſurface of ſnow, ſank 25 degrees below the beginning of Farhenheit's ſcale.

We are told by Dr. Pallas, that in the Deſerts of Siberia, during a very intenſe froſt, the mercury was found congealed in thermometers expoſed to the atmoſphere, and a quantity of that fluid, in an open bowl placed in a ſimilar ſituation, at the ſame time became ſolid. The deciſive experiments which have been made by Mr. Hutchins at Hudſon's Bay prove, that the freezing point of mercury is very nearly 40 degrees below the zero of Farhenheit. From which it follows, that at the time of Dr. Pallas's obſervation, the atmoſphere in Siberia muſt have been cooled to minus 40. By a paper which has been lately tranſmitted to the Royal Society, we are informed, that the ſpirit of wine thermometer in the open air, at Hudſon's Bay, during the winter 1785, fell to minus

42; and from the ſame communication we learn, that by a mixture of ſnow and vitriolic acid, the heat was ſo much diminiſhed, that the ſpirit of wine ſank to minus 80, which is 112 degrees below the freezing point of water.

Hence it is manifeſt, that heat is contained in conſiderable quantities in all bodies, when at the common temperature of the atmoſphere. It is plain, however, that the quantity of heat inherent in each individual body is limited. This I think muſt be admitted, whatever be the hypotheſis which we adopt concerning the nature of heat, whether we conceive it to be a force or power belonging to bodies, or an elementary principle contained in them. For thoſe who conſider heat as an element, will not ſuppoſe that an unlimited quantity of it can be contained in a finite body—and if heat be conſidered as a force or power, the ſuppoſition that finite bodies are actuated by forces which are infinite, is equally inadmiſſible.

To place this in another light, we know that bodies are univerſally expanded by heat, excepting in a very few inſtances, which do not afford a juſt objection to the general fact; becauſe in thoſe inſtances, by the action of heat, a fluid is extricated that previouſly ſeparated the particles from each other. Since, therefore, heat is found to expand bodies in the temperatures which fall within the reach of our obſervation, we may conclude that the ſame thing takes places in all temperatures. It will hereafter appear, that in mercury and in ſome other fluids, the expanſions are proportional to the quantities of heat applied; from which it follows, that the quantities of heat in bodies are limited, becauſe an infinite heat would produce an infinite expanſion.

It is manifeſt that the number of degrees of ſenſible heat, as meaſured by the thermometer and eſtimated from the beginning of the ſcale, muſt be the ſame in all bodies which have a common temperature. For by the firſt general fact it is proved, that heat has a conſtant tendency to diffuſe itſelf uniformly over bodies, till their temperatures become equal. From which it may be inferred, that if a quantity of heat were added to bodies abſolutely cold, the ſame uniform diffuſion would take place; and that if a thermometer, altogether deprived of its heat, were applied to ſuch bodies, it would be equally expanded by them, the whole of the ſenſible heat which they had acquired being indicated by that expanſion.

III. If the parts of the ſame homogeneous ſubſtance have a common temperature, the quantity of abſolute heat will be proportional to the bulk or quantity of matter. Thus the quantity of abſolute heat, in two pounds of water, is double that which is contained in one pound, when at the ſame temperature.

This, I think, is evident, from the ſimilarity in the particles of the ſame homogeneous, ſolid and fluid ſubſtances. For the particles being ſimilar, their powers will be equal, and their capacities for receiving heat will be the ſame; and, therefore, the quantities of abſolute heat which they contain will be proportional to the bulk or quantity of matter.

The truth of the general fact, that bodies of the ſame kind, having equal temperatures, contain quantities of heat proportional to their bulk, is not only manifeſt from the ſimplicity of the operations of nature, but may moreover be confirmed by an induction from the experiments recited in the following work; as the intelligent reader will readily perceive.

IV. The dilatations and contractions of the fluid in the mercurial thermometer, are nearly proportional to the quantities of abſolute heat, which are communicated to the ſame homogeneous bodies, or ſeparated from them, as long as they retain the ſame form. Thus the quantity of heat required to raiſe a body four degrees in temperature, by the mercurial thermometer, is nearly double that which is required to raiſe it two degrees, four times of that required to raiſe it one degree, and ſo in proportion.

The proportions which obtain between the increments of heat in the ſame homogeneous bodies, and the expanſions of the various fluids that have generally been employed in thermometers, were firſt inveſtigated by Mr. De Luc. It was laid down as a principle by this ingenious philoſopher, that if equal quantities of hot and cold water be mixed together, the difference of heat will be equally divided between them. From which it was concluded, that a thermometer being immerſed in the hot water, and alſo in the cold, previouſly to the mixture, if its expanſions were in proportion to the quantities of heat applied, it would point, after the mixture, to the arithmetical mean, or to half the difference of the ſeparate heats, added to the leſs or ſubtracted from the greater. Proceeding upon this principle, he found that when a given quantity of water at 45.5 of Farhenheit, was mixed with an equal quantity of the ſame fluid at 200.75, the mercurial thermometer immerſed in the mixture indicated ſomewhat leſs than the arithmetical mean, the deviation being not more than two degrees below that point. This experiment was repeated at different temperatures with a ſimilar reſult. From which he inferred, that the mercurial thermometer is nearly an accurate meaſure of heat.

As Mr. De Luc's experiments, with the concluſions deduced from them, conſtitute a fundamental part of the doctrine of heat, I have repeated them with as much attention as poſſible.

The thermometers which I employed were mercurial, and had each degree of Farhenheit divided into ten equal parts, being reduced to a common ſcale, by comparing them with a very accurate ſtandard.

EXPERIMENT I.

The air in the room being 61.5, a quantity of water weighing thirteen pounds ten ounces and an half, was heated in a ſlight tinned iron veſſel that had a cover of the ſame metal cloſely adapted to it, a thermometer being inſerted in the center of the cover by means of a cork. When the water was raiſed to the deſired temperature, it was gently agitated, that every part of it might be brought to the ſame heat, the thermometer immerſed in it pointing preciſely to 120.6. An equal quantity of cold water, the parts of which were alſo brought by agitation to a common temperature, was mixed with the warm, by pouring it into the tinned veſſel in which the latter was contained.

When the mixture was reduced by agitating it with a wooden rod to a mean heat, its temperature, at the end of one minute, was 89.8—at the end of ſix minutes, the agitation being continued, it was found to be 89.4. Allowing therefore .066 for the heat loſt in the firſt minute, we have 89.866 for the true temperature of the mixture. If the thermometer, at the moment of immerſion, had indicated the exact arithmetical mean, it would have ſtood at 89.8.

This experiment was repeated with the following reſult.

EXPERIMENT II.

The air in the room being 62, twelve pounds of water at 59.9 were mixed as before with an equal quantity of the ſame fluid at 119.8. The temperature of the mixture, reduced by agitation to a mean heat, was 89.8 at the end of one minute.

An agitation as nearly equal as poſſible being continued, the temperature at the end of five minutes was found to be 89.4. Allowing therefore .08 for the heat loſt in the firſt minute, we have 89.88 for the true temperature of the mixture. The exact arithmetical mean in this experiment was 89.85.

EXPERIMENT III.

The air in the room being 59, twelve pounds of water at 56.1 were mixed as before with an equal quantity of water at

156.25. The mean temperature of the mixture at the end of

1 Minute was 106.1
5 Min. —— 105.3

Allowing therefore .16 for the heat loſt in the firſt minute, we have 106.26 for the true temperature.

In this experiment, which was repeated twice with the utmoſt care, and with very nearly a ſimilar reſult, the arithmetical mean between the ſeparate heats was 106.2.

It appears, therefore, that in the three preceding trials, the true temperature was a little greater than the arithmetical mean. As in theſe trials the mixture was made in a warm veſſel, it is manifeſt, that the deviation from the arithmetical mean is partly to be attributed to the heat received from the veſſel.

The heat imparted by the veſſel was determined by the experiment which follows.

The air in the room being 62, and the veſſel being raiſed to 136, twelve pounds avoirdupois of water at 61.9 were poured into it. The water being agitated, its temperature in

1 Minute was 62.7
5 Min. —— 62.8

The tinned veſſel was heated by including it in a ſimilar veſſel made of copper, the ſize of which was ſuch that the ſurfaces of the veſſels were nearly in contact with each other. Thus circumſtanced, they were placed in a large tub, and were ſurrounded with water at 136; the acceſs of the external air being excluded by a broad pan which covered them, and which contained water nearly of the ſame temperature. By this contrivance the tinned veſſel was entirely ſurrounded with warm water, and received from it an uniform heat, without being in contact with it.

It appears from the experiment, that the veſſel was cooled by the water 73 degrees and .2; and that the water was heated .9 of a degree. And ſince the water received this heat from the veſſel, it is manifeſt, that the ſame quantity of heat which changes the temperature of twelve pounds of water .9 of a degree, will change the temperature of the veſſel 73 degrees and $\frac{2}{10}$. And by a parity of reaſoning, the ſame heat which raiſes the water one degree, will raiſe the veſſel 81.4. If therefore, in any experiment, we find that 81 degrees and $\frac{4}{10}$ of heat have been ſeparated from the veſſel by twelve pounds of water, we may conclude, that this heat has raiſed the water one degree. Hence the heat communicated by the veſſel in the preceding experiments may be determined. Thus in the third experiment the heat of the warm water and of the veſſel was 156.25; the true temperature of the mixture was 106.26. The temperature of the veſſel was therefore ſunk by the cold water 49.99. The increaſe which was produced in the temperature of the cold water by this heat may be determined by the following proportion. If 81.4 give 1, what will 49.99 give? By which it appears, that the water in the experiment in queſtion received from the veſſel .61 of a degree of heat. Subtracting this from 106.26, which was the true temperature of the mixture, we have 105.65 for the temperature which would have been indicated by the thermometer, if no heat had been received from the veſſel. The deviation from the arithmetical mean in this experiment was therefore .55.

In Experiment I. the veſſel was cooled 30.734. To diſcover how much heat the cold water received from the veſſel, ſay, if

81.4 give 1, what will 30.734 give? Hence the heat imparted by the veſſel was .377, which ſubtracted from the true temperature 89.866, gives 89.489 for the temperature, which would have been indicated by the thermometer if no heat had been received from the veſſel.

In Experiment II. from a ſimilar calculation it appears, that if heat had not been communicated by the veſſel, the temperature indicated by the thermometer would have been 89.513. Hence in the former experiment the deviation from the arithmetical mean was .311; in the latter it was .337.

EXPERIMENT IV.

Air in the room	54	
Eight pounds of water at	52.4	
Mixed with an equal quantity of water at	81.9	
Temperature of the mixture in 1 min.	67.1	plus
Three minutes	67.1	
The arithmetical mean in this experiment was	67.15	

In Experiment I. page 20, the quantity of water was twelve pounds, and in that experiment the heat received from the veſſel was .61.

To find how much would have been received, if the quantity of water had been only 8 lb. ſay, as 12 to .61, ſo inverſely is 8 to a fourth proportional, which gives .91 for the heat, which the veſſel would have imparted to 8 lb. of water.

But in the former experiment the veſſel was cooled by the water very nearly 50 degrees. In this experiment it was cooled nearly 15, ſay, as 50 to 15, : : .91 to a fourth, which gives .27 for the heat received from the veſſel.

The true temperature was 67.15, which was alſo the exact arithmetical mean. Hence the deviation from the arithmetical mean was .27.

EXPERIMENT V.

Eight pounds of water -	98.8	
Mixed with 16 lb. of ditto -	54.6	
The mixture in one min. -	69.1	plus
——————— five ——— -	69	plus

When the mixture is ſaid to be 69.1 plus, the meaning is, that the exceſs of its temperature above 69 is a little more than $\frac{1}{10}$. In which caſe it may be ſuppoſed to be $\frac{1}{10}$, plus $\frac{1}{20}$, and the temperature, as indicated by the thermometer, will then be 69.15. Allowing $\frac{1}{50}$ of a degree for the heat loſt in the firſt minute, we have 69.17 for the true temperature.

From the original heat of the veſſel 98.8
Take the true temperature - 69.17
We have - - 29.63

for the heat ſeparated from the veſſel.

To diſcover how much the temperature of the cold water was increaſed by this heat, ſay as 81.4 to 1, ſo is 29.63 to a fourth, which gives .36 for the heat, which would have been communicated by the veſſel to twelve pounds of water.—But the quantity of water was 16 pounds, therefore ſay as 12 to .36, ſo inverſely is 16 to a fourth.—Whence it appears, that the heat imparted to the water by the veſſel was .27, which, ſubtracted from the true temperature, gives 68.9 for the temperature, which would have been indicated by the thermometer if no heat had been received from the veſſel.

The difference between the temperatures of the warm and cold water was 44.2. The quantity of cold water was to that of warm water as 2 to 1, dividing the difference of temperature into three equal parts, if the capacity of water were permanent, and the mercurial thermometer an accurate meaſure of heat, the warm water ſhould have been cooled two of theſe parts, and the cold water heated one. Hence the temperature of the mixture ſhould have been 69.3.

It was before ſhewn, that if no heat had been received from the veſſel, the temperature indicated by the thermometer would have been 68.9.

EXPERIMENT VI.

Eight pounds of water at -	120.8
Mixed with ſixteen pounds of ditto at - -	51.8
Temperature of the mixture	75

In this experiment, the heat imparted by the veſſel was .42 of a degree. We have therefore 74.58 for the temperature, which would have been indicated if no heat had been received from the veſſel.

The temperature, by calculation, is 74.8. Hence, when equal quantities of warm and cold water are mixed together, the mercurial thermometer is found in all caſes to indicate very nearly the arithmetical mean. And, moreover, when the warm and cold water are mixed in different proportions, the reſults correſpond very nearly with the temperatures, as determined by calculation.

I next endeavoured to diſcover, by ſimilar experiments, whether the ſame law obtains when equal quantities of linſeed oil are mixed together at different temperatures.

EXPERIMENT VII.

Air in the room -	56
Ten pounds of linſeed oil at -	145
Mixed with an equal quantity of linſeed oil at -	55.5
The mixture in one minute was	101.7
——— two ———	101.5
——— three ———	101.3
——— four ———	101.1
——— five ———	100.9

In this experiment great attention was required, in order that the heat might be uniformly diffuſed over the oil, previouſly to the mixture, as well as ſubſequent to it.—For when that fluid is expoſed to heat, the warmer particles have a much greater tendency to aſcend to the ſurface than thoſe of water in ſimilar circumſtances. And, conſequently, in the above experiment, a briſk agitation was neceſſary to promote a uniform diffuſion.

If two tenths of a degree be allowed for the heat loſt in the firſt minute, the true temperature of the mixture will be 101.9. Hence the warm oil and veſſel were cooled 43.1, the cold and warm oil being mixed in the veſſel in which the latter was contained. The veſſel was the ſame with that uſed in the former experiments.

The capacity of water is to that of oil very nearly as 2 to 1.—If therefore 81.4 degrees of heat, when ſeparated from the veſſel, raiſe a given quantity of water one degree, the ſeparation of the ſame heat would raiſe an equal quantity of linſeed oil two degrees.

In the experiment in queſtion the veſſel was cooled 43.1.—To diſcover how much the oil was raiſed by this heat, ſay as 81.4

to 2, ſo is 43.1 to a fourth, which gives very nearly one degree for the heat that would have been imparted by the veſſel to 12lb. of linſeed oil, and conſequently the heat communicated by 10lb. was nearly .8 of a degree. Hence we have 100.9 for the temperature, which would have been indicated if no heat had been communicated by the veſſel.—The arithmetical mean is 100.2.

This experiment was thrice repeated with the greateſt attention; and the mean reſult gave nearly .5 for the exceſs of the temperature indicated by the thermometer above the arithmetical mean.

It is probable, that this deviation was in ſome meaſure owing to the tendency of the particles of the warm oil to aſcend to the ſurface, in conſequence of which the difficulty of bringing the ſeveral parts to a uniform heat, and of marking the mean temperature, was very great. And, for the ſame reaſon, the reſults of the ſeveral experiments with that fluid could not be brought to correſpond ſo nearly as the reſults of thoſe which were made with water. If, however, we ſuppoſe the experiments to be exact, and the thermometer to have been an accurate meaſure of heat, it will follow that the capacity of linſeed oil is a little augmented by an increaſe of temperature.

The reader will perceive, that in the foregoing experiments with water, when equal portions of this fluid, having different degrees of ſenſible heat, were mixed together, the temperature of the mixture, as indicated by the mercurial thermometer, was a little leſs than the arithmetical mean.

This irregularity Mr. De Luc accounts for in the following manner: He ſuppoſes, that the contractions of mercury have an increaſing progreſs compared with the diminutions of heat in bodies—that a double diminution of heat, for example, will produce more than a double contraction; a triple diminution more than a triple contraction. In conſequence of which the mercurial thermometer, in experiments ſimilar to thoſe recited above, muſt indicate leſs than the arithmetical mean.

Mr. De Luc has, however, himſelf obſerved, in a paper, with which he ſome time ago favoured me on this ſubject, that we cannot determine with certainty from thoſe experiments, the relation which the expanſions of mercury bear to the increments of heat. For when we infer the agreement between the dilatations of mercury and the increments of heat from ſuch experiments, we take it for granted, that the capacity of water for receiving heat, continues permanent at all temperatures between the freezing and boiling points. This, however, ſhould not be admitted without proof.

It is proper to add, that if the mercury in the experiments in queſtion had indicated the preciſe arithmetical mean, we could not from thence infer with certainty, that the dilatations of this fluid are proportional to the increments of heat. For ſuch alterations may be conceived to take place in the capacity of water, and in the contractions or dilatations of mercury, in conſequence of a change of temperature, as would account for the phenomenon, though the expanſions of mercury and the increments of heat were not proportional to each other. If, for example, unequal augmentations of capacity are produced in water by equal increments of heat;

and if unequal contractions are produced in mercury by equal diminutions of heat, theſe contrary irregularities may be conceived to be ſo adjuſted as preciſely to balance each other, in conſequence of which a mercurial thermometer introduced into equal quantities of hot and cold water mixed together, would point to the arithmetical mean.

As theſe experiments, therefore, are not deciſive, I have endeavoured to diſcover the relation between the expanſions of mercury and the increments of heat, by a mode of trial which appears leſs liable to objection.

In the courſe of my experiments on heat, I obſerved, that if the vapour of boiling water be made to flow in a continued ſtream through a pipe of any length or dimenſions, every part of that pipe will be raiſed to the boiling point; and, moreover, that if the extremity of a pipe, leading from a covered veſſel containing boiling water, be inſerted into another covered veſſel, which has two ſmall apertures for the purpoſe of ſuffering the ſteam to enter and to eſcape, every part of the latter veſſel will be kept at the temperature of boiling water, provided the vapour be introduced in ſuch a quantity, as that a portion of it ſhall eſcape uncondenſed. In reflecting on this fact, it occurred to me, that a method might be deviſed of combining the freezing and boiling points of water, ſo as to obtain an intermediate temperature that ſhould be conſtant. With that intention, I contrived the following apparatus; ſee plate I. fig. 1.

A C repreſents a double veſſel conſiſting of two cylinders, A B C D, and *l m n o*, which are joined at the ſuperior extremity (*l o*), the ſpace between them being compleatly cloſed, excepting at the two apertures C and B, into the former of which the ſteam of boiling water enters, and the latter ſuffers it to eſcape.

In this veſſel, therefore, there are two compartments, one of which is the ſpace intercepted between the interior and exterior cylinders, and the other is that included within the interior cylinder, which is open at (*l o*), and cloſed at the bottom (*m n*). The ſteam that paſſes from the boiler G H through the pipe G C, into the ſpace between the cylinders which forms the exterior compartment, keeps every part of that ſpace at

the

the boiling point, as long as a portion of uncondenſed vapour iſſues from the pipe B.

Upon the veſſel A C is inverted another double veſſel E D, conſiſting of the interior and exterior cylinders E A D F, and *r p s q*, which are joined at the lower extremity *p q*. In this veſſel, therefore, there are alſo two compartments, one of which is formed by the ſpace between the cylinders, which is open at its ſuperior extremity E F, and the other by that included within the interior cylinder; which being cloſed at its ſuperior extremity (*r s*), and open below, communicates with the interior compartment of the lower veſſel. The exterior compartment of the upper veſſel is filled with pounded ice, which is conſequently applied to the external ſuperficies of the interior cylinder, as well as to its bottom, (*r s*), being introduced in ſuch a quantity as compleatly to occupy the cavity *e p r s q f*.

The two interior cylinders *l m n o* and *p r s q*, are equal in ſize. Between the ſuperior and inferior veſſels, for the purpoſe of interrupting the ſudden communication of the heat, is placed a thin flat annular piece of cork, on which the ſuperior veſſel reſts, the diameter of the cork being equal to the breadth of the inner compartment.

The exterior cylinder A E F D of the upper veſſel, is continued as far as the points A and D; and the outer ſurface of this veſſel, being broader than that of the inferior one, by the above-mentioned continuation, a cavity is formed, which is filled with plaſter of Paris; and thus the two veſſels are cemented together. Through a perforation in the cork placed between the veſſels, a ſmall tube paſſes, one extremity of which opens at the point *o*, into the compartment formed by the interior cylinders; and the other extremity extending through the plaſter of Paris, reaches to a ſmall diſtance beyond the outer cylinder of the ſuperior veſſel. In this tube a thermometer (*t v*) is inſerted, which has the mercury contained in a thin cylinder, nearly equal in length to the tranſverſe diameter of the inner compartment.

As the interior cylinders are equal in ſize, and as the lower veſſel is kept in all its parts at the boiling, and the upper veſſel at the freezing point, it may be inferred, that the thermometer (*t v*), which paſſes horizontally through the center of the ſpace intercepted ed between the cylinders, and which is conſequently expoſed to equal ſurfaces at 32 and at 212, will acquire a degree of heat that is preciſely the arithmetical mean between thoſe points; and if the fluid in the thermometer indicate likewiſe the arithmetical mean, we may conclude, that the expanſion and the heat acquired are proportional to each other.

The veſſel that has the temperature of 32, is placed over that which is at the boiling point, in order that an uniform diffuſion of the heat may be produced, by the cold air deſcending from above, and intimately mixing with the warmer air below*.

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* Upon a principle ſimilar to that which has been employed in the apparatus deſcribed above, I have contrived an inſtrument, by means of which fixed temperatures may be obtained at all the intermediate degrees of heat between the freezing and boiling points of water.

This inſtrument, like that which has been now deſcribed, conſiſts of two double veſſels, one of which is inverted upon the other, the ſuperior being kept at 32 by means of pounded ice, and the inferior at 212 by the ſteam of boiling water. The interior cylinders of theſe veſſels are formed of braſs poliſhed in the inſide; and in each cylinder a moveable piſton is placed; that which is inſerted in the upper being kept at 32, and that in the lower at 212. Hence it is manifeſt, that by changing

The relation between the expanſions of mercury and the real increments of heat, as determined by this apparatus, will appear from the following experiment.

EXPERIMENT VIII.

The cylindrical mercurial thermometer deſcribed above, was inſerted by means of a cork in the tube *g*, and the middle point of the cylinder containing the mercury was made to coincide as nearly as poſſible with the

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the relative poſitions of the piſtons, the proportions of the ſurfaces at 32 and at 212, which are intercepted between them, may be varied at pleaſure.

When each piſton is raiſed to the upper extremity of the cylinder in which it is contained, we ſhall then have the whole of the ſuperior cylinder, which is at 32, expoſed to the action of the inferior piſton at 212. If we wiſh to have a degree of heat lower than this, two braſs plates, having the temperature of 32, may be introduced horizontally at the bottom of the upper cylinder, and may be gradually made to ſlide over the ſurface of the lower piſton, until at length they meet. Each of theſe plates ſhould conſiſt of two parallel plates, which are placed at a little diſtance from each other, and which are joined at their extremities by croſs plates, the ſpace intercepted between them being filled with pounded ice, for the purpoſe the center of the ſpace included within the interior cylinders. The exterior compartment of the upper veſſel was then filled with ſnow at 32, and the outer compartment of the lower veſſel was raiſed by the ſteam of boiling water to 212. In a ſhort time the thermometer roſe to 121, where it remained without variation during the ſpace of 15 minutes, the experiment being then diſcontinued. When this experiment was made, the barometer ſtood at 29.6. The boiling point of the thermometer had been taken when the barometer was at 30 inches.

poſe of retaining them at the fixed temperature of 32. By this means, a ſmaller portion of the ſurface of the lower piſton will be expoſed to the upper cylinder, in proportion as the plates approach towards each other, and when they meet, it will be wholly covered by them. The air in the upper cylinder will be then reduced to the freezing point, as it will be entirely ſurrounded by ſurfaces at 32. A ſimilar apparatus may be employed to raiſe the air in the lower cylinder, by regular gradations, to the temperature of 212.

It is manifeſt that, upon the ſame principle, by means of the freezing, and boiling, points of other ſubſtances, inſtruments may be made for exhibiting fixed degrees of heat, which ſhall comprehend a much larger ſcale than the inſtrument now deſcribed.

In Experiment III. the deviation of the mercury from the arithmetical mean was .55 of a degree; the difference of temperature between the hot and cold water, previouſly to the mixture, having been nearly 100 degrees. Hence we may infer, that if the difference of temperature had been 180, the deviation would have been .99, which coincides very nearly with the reſult of the foregoing experiment.

When this experiment was repeated with unequal ſurfaces, the central heat was not found accurately to vary with the ſurfaces; the effect produced by the ſmaller ſurface being comparatively greater than that produced by the larger. For when the ſurface at 32 was ſo diminiſhed as to become one half of that at 212, the heat at the centre, as might be expected, was increaſed; but the *increaſe* of the central heat was leſs than the diminution of the cooling ſurface. On the contrary, when the ſurface at 212 was diminiſhed till it became one half of that at 32, the *diminution* of the central heat was leſs than that of the heating ſurface. The reaſon of which ſeems to be, that the ſuperior and inferior ſurfaces have greater power in heating or in cooling the air than the lateral ſurfaces. For a current of warm air is continually aſcending, by the lateral ſurfaces, and a contrary current of cold air is deſcending through the centre, as I have found by experiment; and the force with which theſe currents are impelled againſt the ſuperior and inferior ſurfaces, by increaſing the points of contact, increaſes the heating and cooling powers. In theſe experiments the capacity of the leſſer veſſel was diminiſhed by ſhortening its lateral ſurface: hence the perpendicular altitudes of the two veſſels were unequal, but their breadth was the ſame, and conſequently the ſuperior and inferior ſurfaces, on which the heating and cooling power principally depended, being equal, the heats did not vary in proportion to the variation in the ſize of the veſſels. It is proper to add, that the refrigerating power of the colder ſurface was found to be ſomewhat greater than the heating power of the warmer, in ſimilar circumſtances. The cauſe of this difference it is difficult to determine; but from whatever cauſe it may have ariſen, it leads to the following concluſion: ſince it is found that in ſimilar circumſtances,

cumſtances, when the ſurfaces are unequal, the refrigerating power of the colder, is greater than the heating power of the warmer ſurface, it may be inferred, that if the ſurfaces be made to approach towards equality, the effect produced by the colder ſurface will not in any intermediate point be leſs than that produced by the warmer. Hence we may conclude with certainty, that when the ſurfaces become equal, the real heat applied to a thermometer that paſſes horizontally through the centre of the apparatus, will not be greater than the true arithmetical mean between the freezing and boiling points.

It is poſſible, however, that it may be a little leſs; for the refrigerating power of the ice in melting, may be greater than the heating power of the ſteam during its condenſation. But it is, I think, evident, that the deviation produced by this cauſe, ſuppoſing it to exiſt, muſt be inconſiderable: for in order that two ſubſtances ſhould tranſmit heat unequally, it is neceſſary either that their magnitudes, conducting powers, or capacities for heat ſhould be unequal, or that the differences between their heats, and thoſe of the ſubſtances with which they are in contact, ſhould be unequal. But in the above apparatus the heat paſſes from the ſteam to the ice, through ſubſtances which have equal magnitudes, capacities, and conducting powers. If, therefore, in the experiment in queſtion, the heat had been tranſmitted unequally, the difference between the temperature of the ſuperior cylinder and of the ice muſt have been greater or leſs than the difference between the temperature of the inferior cylinder and of the ſteam: that is, the inferior cylinder muſt have been more or leſs heated by the ſteam, than the ſuperior cylinder was cooled by the ice. But as the cylinders are good conductors of heat, and are cloſely in contact with the ice and ſteam, which have fixed temperatures, it is evident, that they muſt acquire very nearly the temperatures of 32 and 212. If, however, from the greater refrigerating power of the ice, we conceive the difference between its temperature and that of the ſuperior cylinder, to have been half a degree leſs than the difference between the temperature of the ſteam and of the inferior cylinder, the real heat at the centre would not be reduced by the operation

tion of this cauſe more than a quarter of a degree below the arithmetical mean.

The thermometer in Experiment VIII. ſtood at 121, which was one degree leſs than the arithmetical mean. And ſince it has been ſhewn, that the real heat, at the centre of the apparatus, in that experiment, was not greater than the arithmetical mean, and that it did not deſcend below it more than $\frac{1}{4}$ of a degree, we hence conclude, that the thermometer, in the experiment in queſtion, did not deviate from the real heat more than *one* degree, nor leſs than $\frac{3}{4}$ of a degree.

It is proper to obſerve, that the boiling point of the cylindrical thermometer uſed in this experiment, was taken by immerſing it in boiling water, when the barometer ſtood at 30 inches, the cylinder and a ſmall part of the tube being covered with the water. Now if we ſuppoſe the expanſions of mercury accurately to correſpond with the real heats, it is manifeſt that a thermometer, which is thus graduated, muſt obſerve a decreaſing progreſs, compared with the increments of heat. This clearly appears from the experiments and obſervations contained in Mr. Cavendiſh's excellent paper on the method of graduating thermometers*. For let a thermometer, graduated as above, be conceived to be immerſed in water at 32, the air in the room being alſo at 32, and let the tube be ſunk to ſuch a depth in the water, that the ſurface of the latter ſhall coincide with the 32d degree of the thermometer. If the temperature of the water be gradually increaſed, the thermometer being kept in the ſame poſition, a portion of the mercury will riſe above the ſurface; and if at the ſame time the temperature of the air be not increaſed, that portion of the mercury which is expoſed to its influence will have its expanſion diminiſhed.

Let the water, for example, be raiſed to 33, the air remaining at 32: it is manifeſt, that the mercury, which, by its aſcenſion in the tube, is now expoſed to the influence of the air, will be leſs expanded than if it were wholly immerſed in the water †.

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* See Philoſophical Tranſactions, Vol. LXVI. p. 380.

† See the Report of the Committee for adjuſting Thermometers, publiſhed in the Philoſophical Tranſactions, Vol. LXVII. p. 516.

If the difference between the temperatures of the air and water be increaſed, the diminutions produced in the expanſion of that part of the mercury which is ſurrounded by the air will likewiſe increaſe. And hence, in proportion as the thermometer riſes, greater quantities of heat muſt be continually added to the water, in order to produce equal expanſions in the mercury. The latter will therefore obſerve a decreaſing progreſs, compared with the increments of heat.

Agreeably to this I have found, that when the cylinder and tube uſed in Experiment VIII. were completely ſurrounded with ſteam, according to the method recommended by Mr. Cavendiſh, the mercury roſe nearly a degree and three fourths above the boiling point, which had been taken in the common method. And the ſame thermometer, at the intermediate diviſion between the freezing and boiling points, ſtood nearly one degree lower than a thermometer which was graduated according to Mr. Cavendiſh's ideas, and which was completely immerſed in water.

If allowance be made for this error, it will follow, that in Experiment VIII. the mercurial thermometer indicated very nearly the arithmetical mean.

The thermometers employed in the experiments with warm and cold water, recited above, were reduced to the ſame ſcale, by comparing them with a ſtandard, which, as well as that uſed in Experiment VIII. had the boiling point taken when the barometer ſtood at 30 inches, the bulb and a ſmall portion of the tube being immerſed in the water. Theſe thermometers muſt, therefore, as was already ſhewn, neceſſarily obſerve a decreaſing progreſs, compared with the real increments of heat. And hence we find, that, in the experiments alluded to, the thermometer which was employed to determine the temperature of the mixture, ſtood a little lower than the arithmetical mean. When this error was corrected, it appeared that the preciſe arithmetical mean was very nearly indicated in thoſe experiments.

In making experiments on the heat of fluids, the cauſe of error now explained may be entirely avoided, by graduating the thermometer according to the method recommended by Mr. Cavendiſh, and by immerſing it, in each experiment, to a depth equal to the elevation of the mercury. But in many experiments this is attended with great inconvenience, and in ſome it is impoſſible.

If the thermometer be graduated in Mr. Cavendiſh's method, and it be not in each experiment entirely immerſed, the error will be increaſed: it will fall more below the real heat, than if a thermometer made in the common way had been employed.

It ſeems beſt, therefore, in experiments that do not admit of a total immerſion, to employ thermometers graduated in the common method, and at the ſame time to find the boiling point by the method which Mr. Cavendiſh has propoſed. And thus, as has been ſhewn by that philoſopher, a ſcale of the errors may be formed, by which we ſhall be enabled to make a near approximation to the truth. But this ſcale of errors will not be perfectly accurate, unleſs in all experiments, the thermometer be immerſed to the ſame depth, and the air have the ſame temperature that it had when the boiling point was originally taken. The inaccuracies, however, produced by changes of temperature in the air, if the experiments be not made in the extremes of heat or cold, will be inconſiderable; and it is poſſible to make an additional ſcale of the errors ariſing from that cauſe, which may be employed in experiments that require very great accuracy.

The reader will obſerve, that there is a difference of nearly two degrees between the general reſult of the foregoing experiments, and thoſe of Mr. De Luc on the ſame ſubject.

According to that philoſopher, the mercurial thermometer, when it has acquired the degree of heat which is the arithmetical mean between the freezing and boiling points, ſtands at 38.6 of his ſcale, which is 118.85 of Fahrenheit's. By the experiments related above it ſtands at 121. The cauſe of this difference cannot eaſily be determined with certainty. Mr. De Luc himſelf ſuppoſes, that the real deviation from the arithmetical mean is not altogether ſo great as it appeared in his experiments, a ſmall error being conceived to have ariſen from the evaporation of the heated water, and allowance not having been made, excepting in one experiment, for the refrigeration produced by the external air. In the experiments with warm and cold water recited above, particular attention was paid to theſe circumſtances; and as the thermometers which I employed were made with great care, and were reduced to a common ſcale, by comparing them with an accurate ſtandard, I cannot help conſidering the reſults of theſe experiments as approaching nearly to the truth. And I am much confirmed in this concluſion, by their coincidence with Experiment VIII. which was made in ſuch different circumſtances, and in a manner ſo little liable to error.

The following experiment was made to determine the relation between the expanſions of *water* and the increments of heat, by means of the apparatus uſed in Experiment VIII.

EXPERIMENT IX.

A water thermometer, that had the water contained in a thin flat tube, was inſerted in the apparatus, the center of the tube being made to coincide with the center of the ſpace intercepted between the interior cylinders, as in the former experiment.

In the courſe of a few minutes the water roſe to 78, where it remained without variation.

For the purpoſe of making this thermometer, a tube was choſen which was nearly accurate, and the ſlight inequalities that occurred in it were compenſated by the ſize of the diviſions in the ſcale. The water, previouſly to its introduction into the tube, was freed from air, that it might bear the temperature of 212. It had the boiling point taken when the barometer was at 30, and the freezing point was found, by immerſing it in water, cooled to 32.

The reſult of this experiment very nearly correſponds with that of Mr. De Luc on the ſame ſubject. For by Mr. De Luc's experiment, when the mercurial thermometer ſtood at 40 of his ſcale, (equal to 122 of Fahrenheit's) the water thermometer ſtood at 20.5 of his ſcale, which was equal to 78.125 of Fahrenheit's.

From Experiment VIII. it appears, that when the mercurial thermometer is expoſed to a degree of heat that is intermediate between the freezing and boiling points of water, it indicates very nearly the arithmetical mean. Hence we may conclude, with great probability, that the expanſions of mercury are proportional to the quantities of heat applied, and conſequently that the mercurial thermometer is an accurate meaſure of heat. And this concluſion is, I think, confirmed by the coincidence between the abovementioned experiment, and thoſe with warm and cold water formerly recited.

It moreover appears from Experiment IX. that the expanſions of *water* do not correſpond with the real increments of heat, the deviation from the arithmetical mean between the freezing and boiling points, in that experiment, being 44 degrees of Fahrenheit's ſcale.

5. The capacities of bodies for containing heat are nearly permanent, as long as they retain the ſame form.

The capacity of a body for heat is ſaid to be permanent, when the ſame quantity of abſolute heat which raiſes it one or two degrees, as meaſured by an equidifferential thermometer, at a given temperature, will raiſe it an equal number of degrees at all other temperatures. On the contrary, the capacity is ſaid to be increaſed or diminiſhed, by an alteration of temperature, when in conſequence of ſuch an alteration a greater or a leſs quantity of abſolute heat muſt be applied to it, in order to produce an equal effect upon the thermometer.

It has been proved by Experiment VIII. that the mercurial thermometer is nearly an accurate meaſure of heat; and from the experiments with warm and cold water, it appears, that when equal portions of this fluid are mixed together at different temperatures, the mercurial thermometer indicates very nearly the arithmetical mean. Hence it follows, that the capacity of water is permanent, in all the intermediate temperatures, between the freezing and boiling points. For if, by augmenting its ſenſible heat, the capacity of water were increaſed, an equidifferential thermometer, in experiments ſimilar to thoſe recited above, would point to *more*, and if diminiſhed, to *leſs* than the arithmetical mean.

The truth of this will appear from the following propoſitions.

PROPOSITION I.

If two equal portions of the ſame fluid be mixed together at different temperatures, the heat will be equally divided between them; or, in other words, the quantity of abſolute heat imparted by the warmer to the colder fluid, will be half the difference of the ſeparate heats.

There are three caſes of this propoſition: 1. The capacity of the fluid may be continually increaſed or continually diminiſhed by an augmentation of temperature. 2. It may be alternately increaſed and diminiſhed. 3. The capacity may remain permanent in all temperatures, while the fluid retains the ſame form *.

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* In the two former of the above caſes, the variation of capacity is ſuppoſed to be gradual. It is poſſible, however, that the capacity of a body may be *ſuddenly* varied by a change of temperature. Thus it has been proved by Dr. Irvine, that when water has arrived at the boiling point, it undergoes an inſtantaneous change of capacity in the moment of its converſion into vapour. But the ſuppoſition of a *ſudden* variation of capacity cannot have place in the preſent inſtance; becauſe when the capacities of bodies are ſuddenly varied by an alteration of temperature, they are found to ſuffer a change of form, as appears from the diſcoveries of the above-mentioned philoſopher. And conſequently, if the temperature be altered while the form continues the ſame, the capacity muſt either be *permanent*, or it muſt be *gradually* increaſed or diminiſhed.

Caſe 1. Let A and B be equal portions of any fluid, the capacity of which is conceived to be continually increaſed by a gradual augmentation of its temperature. Let the part A be ſuppoſed to have a higher temperature than the part B. It is affirmed, that if B and A be mixed together, and brought to a common temperature, A will communicate to B one half of the exceſs of its heat above the original heat of B.

For let the temperatures of A and B (ſee plate I. fig. 2 and 3.) be repreſented by the lines C H and L M; and let the ordinates *u v*, *w x*, *y z*, and *a b*, *c d*, *e f*, be drawn repreſenting the capacities at different temperatures, the capacities at the point of total privation being expreſſed by the lines B C, K L; and let B G and K I be the lines joining the extremities of the ordinates.

It was obſerved above, that the quantities of abſolute heat in bodies are proportional to the rectangles under the capacities and temperatures eſtimated from the point of total privation. If, therefore, the lines C H, L M, repreſent the temperatures, and the lines H G, M I, the capacities, the abſolute heats will be repreſented by the rectangles H R, HR, MQ. When the bodies are mixed together, and brought to a common temperature, let the heat of A be diminiſhed by the figure H E R, and let that of B be increaſed by the figure O I P; and ſince the heat which is taken from A is the ſame with that which is added to B, it is manifeſt that H E R muſt be equal to O I P. From the third general fact, it is evident, that when A and B are brought to the ſame temperature, they contain equal quantities of abſolute heat. Therefore the figure F C S E, is equal to the figure O L P N. For the ſame reaſon the figure D T is equal to the figure M Q: conſequently the remainder O I P or H E R is equal to the remainder F A S. That is, A communicates to B one half of the exceſs of its heat above the original heat of B*.

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* In the reaſoning which has been employed to eſtabliſh the above propoſition, it is taken for granted, that no part of the heat which is abſorbed by bodies, in conſequence of an increaſe of capacity, enters into a chemical union with them.

The opinion of thoſe philoſophers who ſuppoſe that elementary fire is capable of chemically combining with bodies, does not appear to be founded upon deciſive experiments. I ſhall endeavour to ſhew in the ſequel, that we have no ſufficient evidence for believing that bodies do, in any inſtance, enter into this peculiar mode of combination with that element.

If, therefore, any fluid have its capacity continually augmented by an increaſe of temperature, and if equal portions of this fluid be mixed together at different temperatures, the quantity of abſolute heat imparted by the warmer to the colder fluid will be half the difference of the ſeparate heats.

In like manner it may be proved, that if a fluid have its capacity continually diminiſhed by an increaſe of temperature, and if equal portions of it be mixed together at different temperatures, the heat will be equally divided between them.

If the capacity be alternately increaſed and diminiſhed by augmenting the temperature, as in the ſecond caſe, the line joining the extremities of the ordinates which repreſent the capacities will be a waving line. If the capacity continue permanent, as in the third caſe, it will be a ſtraight line. The demonſtration of theſe caſes is the ſame with that of the firſt. We may therefore conclude univerſally, that if two equal portions of the ſame fluid be mixed together at different temperatures, the quantity of abſolute heat communicated by the warmer to the colder fluid, will be half the difference of the ſeparate heats.

PROPOSITION II.

If a fluid have its capacity continually increaſed by an augmentation of temperature, and if equal portions of that fluid, having different temperatures, be mixed together, the temperature of the mixture, as meaſured by an equidifferential thermometer, will be greater than the arithmetical mean: or in other words, the ſenſible heat imparted by the warmer to the colder fluid, will be more than half the difference of the ſeparate heats.

For as the capacity of the fluid increaſes with the temperature, the quantities of abſolute heat which are required to produce equal alterations of temperature will be continually increaſing. Conſequently, if the difference between the temperatures of the warmer and colder fluid be conceived to be divided into two equal parts, that is, if D F be equal to F H, (ſee plate I. fig. 2.) the quantity of abſolute heat required to raiſe the fluid from D to F, will be leſs than that required to raiſe it from F to H. From which it follows, that if the line D H, repreſenting the exceſs of the temperature of the warmer above that of the colder fluid, be conceived to be ſo divided in the point F, that the quantity of abſolute heat which raiſes the fluid from D to F, ſhall be equal to that which raiſes it from F to H, H F muſt be leſs than F D. Hence if we take from the warmer fluid a quantity of heat, by which the exceſs of its abſolute heat above that of the colder, ſhall be diminiſhed one half, the difference of temperature will be diminiſhed *leſs* than the half. In like manner it may be proved, that if we add to the colder fluid a quantity of abſolute heat, equal to that which has been taken from the warmer, or to half the difference of the ſeparate heats, its temperature will be augmented *more* than the half. But it was before ſhewn, that when the fluids are mixed together, the quantity of abſolute heat which the warmer imparts to the colder, is preciſely half the difference of the ſeparate heats. From which it follows, that the diminution in the temperature of the warmer fluid will be leſs than the increaſe in that of the colder, and hence the temperature of the mixture will be greater than the arithmetical mean.

By a ſimilar mode of reaſoning, it may be ſhewn, that if a fluid have its capacity continually diminiſhed by an increaſe of temperature; and if equal portions of that fluid, having different degrees of ſenſible heat, be mixed together, the temperature of the mixture will be leſs than the arithmetical mean.

If the capacity were irregularly changed by an alteration of temperature, being in ſome parts of the ſcale increaſed, and in others diminiſhed; and if the warmer fluid, previouſly to its mixture with the colder, were raiſed to a variety of different temperatures in a ſeries of experiments ſimilar to thoſe recited above; in that caſe, an equidifferential thermometer would, in ſome experiments, indicate more, and in others leſs, than the arithmetical mean.

It appears, however, that in Experiments I, II, III, and IV. the arithmetical mean was very nearly indicated by the mercurial thermometer. And from Experiment VIII. it is inferred, that this inſtrument is nearly an accurate meaſure of heat. And ſince it has been ſhewn, that if the capacity of water were either *regularly* or *irregularly* varied by an alteration of temperature, a thermometer which was an accurate meaſure of heat, would, in thoſe experiments, have indicated more or leſs than the arithmetical mean, it follows, that the capacity of this fluid is permanent in all temperatures between the freezing and boiling points.

I may add, that the truth of theſe concluſions is much confirmed by the coincidence between the reſults of the above-mentioned experiments, and that of experiment VIII.

For if we ſuppoſe the capacity of water to vary in different temperatures, the reſults of the firſt ſeven experiments will oblige us to have recourſe to the very improbable ſuppoſition, that theſe variations are preciſely balanced by contrary variations in the expanſions of mercury. And again, if we imagine the expanſions of mercury to vary, the reſult of the 8th experiment obliges us to ſuppoſe, that this variation is balanced by a deviation of the true heat, at the center of the apparatus uſed in that experiment, from the arithmetical mean; which is alſo very improbable. But that a deviation from the true heat ſhould moreover take place in the center of this apparatus,

paratus, preciſely equal to the ſuppoſed variations in the capacity of water, implies ſuch a coincidence of improbable events, that I think the contrary may be conſidered as certain.

The permanency of the capacity of water being proved, and the mercurial thermometer being ſhewn to be an accurate meaſure of heat, the influence of a change of temperature upon the capacities of other bodies, may be determined by comparing them with water as a ſtandard. To illuſtrate this, let equal bulks of water and iron be ſuppoſed to be mixed together, the latter having a higher temperature than the former by fifty degrees. Let them be conceived to be mixed a ſecond time, the exceſs of temperature in the iron being 100 degrees; and let the ſenſible heat loſt by the iron in the firſt inſtance be equal to that gained by the water. If the capacity of iron be permanent, the ſenſible heat which it loſes in the ſecond inſtance will alſo be equal to that which the water receives; but if its capacity be increaſed, the diminution of temperature in the iron will be leſs, and, if diminiſhed, *greater* than the augmentation of temperature in the water.

I have mixed moſt of the metals and their calces with water at different temperatures; and from the general reſult of my experiments on this ſubject, I have the utmoſt reaſon to believe, that the capacities of bodies, while they retain the ſame form, are not liable to be varied by a change of temperature. For though many of the trials did not correſpond accurately with that ſuppoſition, yet they agreed more nearly with it, than with any other. From which we may infer, that the ſuppoſition is true.

It is upon a mode of reaſoning ſimilar to this, that moſt of our concluſions are founded, reſpecting the exiſtence of general laws. Aſtronomers, for example, do not conclude, that the heavenly bodies move in elliptical orbits, becauſe their obſervations indicate that path preciſely, but becauſe they approach more nearly to it, than to the circular, or to any other line of motion. At the ſame time it muſt be acknowledged, that though perfect accuracy cannot be attained, yet in proportion as experiments make a nearer approximation to that point, the inferences deduced from them will be *more* certain, and *leſs* labour will be required in tracing them to firſt principles.

From the foregoing experiments and obſervations, we may conclude in general, that the capacities of bodies which retain the ſame form, are permanent in the intermediate temperatures, between the freezing and boiling points of water. Hence we may infer by induction, that the ſame law obtains throughout the whole of the ſcale of heat.

The reliance which we are diſpoſed to place upon the evidence of inductive reaſoning, is confirmed by the univerſal experience of mankind, which proves that the operations of nature are ſimple and uniform, that ſimilar effects proceed from the agency of ſimilar cauſes, and that all the phænomena of the univerſe, as far as they fall within the reach of human obſervation, are governed by general laws. Thus if it be found by experiment and obſervation, that the forces of the ſun's attraction to ſome of the planets are inverſely as the ſquares of the diſtances, we infer, from the ſimplicity of the operations of nature, that this law prevails throughout the whole of the planetary ſyſtem, and governs the motions of all the bodies that revolve around the ſun as a center. In like manner if it be found, in thoſe temperatures which come within the reach of our obſervation, that while the forms of bodies continue the ſame, the capacities are permanent, we conclude by induction, that this is a general law of nature, which extends throughout the whole of the ſcale of heat, even to the point of total privation.

As this concluſion is founded upon an extenſive ſeries of experiments, and as it affords a ſimple interpretation of the phænomena, it may, I think, be conſidered as a legitimate induction. And if any deviation from the general law ſhould afterwards appear in particular inſtances, it muſt be admitted with ſuch exceptions as occur *.

By the permanency of the capacities of bodies for heat, we are enabled to account for the regularity with which they give off heat when placed in a cold medium.

The laws of the refrigeration of heated bodies were firſt inveſtigated by Sir Iſaac Newton. This illuſtrious philoſopher ſuppoſed, with great probability, that the quan-

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* See Newton's Optics.

tities of heat loſt by a body in ſmall portions of time, would always be proportional to the exceſs of its temperature above that of the ſurrounding medium. Thus, if the heated body were 180 degrees warmer than the atmoſphere, the quantity of heat which it would loſe in a given moment, would be double that which it would loſe in an equal portion of time, if it were only 90 degrees warmer than the atmoſphere. From which it follows, that if the times were taken in arithmetical progreſſion, the decrements of heat would be in geometrical progreſſion, and the heats remaining, conſidered as the differences between the temperature of the body and of the external air, would alſo obſerve the ſame law.

Dr. Martine has ſhewn, that this rule, though nearly accurate, is not to be admitted without ſome reſtriction. From *his* experiments, compared with thoſe of Muſchenbroke, it appears, that the decrements are in a proportion ſomewhat greater than the remaining heats, and conſequently the quantities of heat loſt are partly equable, and partly in geometrical progreſſion.

It appears, therefore, that bodies are cooled according to the ſame fixed laws: from which it follows, that we muſt either admit the capacities to be permanent, or muſt have recourſe to the very improbable ſuppoſition, that the changes of capacity produced by alterations of temperature, are preciſely ſimilar in all the various claſſes of natural bodies.

For if the changes of capacity were diſſimilar, the laws of refrigeration would vary in different ſubſtances. To illuſtrate this, let B and C be two bodies, the former of which has its capacity uniformly diminiſhed, and the latter uniformly increaſed by a diminution of temperature. If theſe bodies be heated and placed in a cold medium, the refrigeration of the firſt will be proportional to the *joint effect* of two cauſes which act in conjunction with each other: for B will be cooled both by the refrigerating power of the ambient medium, and by the abſorption of heat ariſing from the increaſe of its capacity: the refrigeration of the ſecond, on the contrary, will be proportional to the *difference* between two cauſes which act in oppoſition to each other, for C will be continually deprived of heat by the cooling power of the ſurrounding medium, and it will be continually ſupplied with heat from an internal ſource, in conſequence of the diminution of its capacity. Hence the cooling power of the ambient medium, will in the firſt inſtance be aided by the abſorption of heat; and in the ſecond inſtance, it will be counteracted by the exhication of that principle. The quantities of heat loſt therefore by B, in ſmall intervals of time, will be in a *greater* proportion than the heats remaining, conſidered as the differences between its temperature and that of the external air; and the quantities loſt by C will be in a *leſs* proportion than the hearts remaining *. The laws of refrigeration in B and C will therefore be different. But theſe differences are not found to take place in nature.

It has moreover been proved, that the laws of the refrigeration of water, are the ſame with thoſe which obtain in the cooling of other fluid as well as ſolid ſubſtances. And ſince it has been ſhewn by experiment, that the capacity of water for containing heat is permanent, we may infer, that in other ſubſtances, while the forms are unchanged, the capacities are alſo permanent.

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* It is here ſuppoſed agreeably to the rule laid down by Sir Iſaac Newton, that if the capacities were permanent, the quantities of heat carried off by the ſurrounding medium in ſmall ſucceſſive intervals of time, would be in geometrical progreſſion.

Future experimentaliſts may perhaps diſcover ſome exceptions to this law. From the cold produced by the ſudden expanſion of aerial fluids, it is probable, that the capacities of thoſe ſubſtances are a little increaſed by an augmentation of temperature; but it will be ſhewn in the ſequel, that if ſuch exceptions to the general law do exiſt, the effects which they produce are very inconſiderable.

It was formerly proved, that in the ſame homogeneous bodies, if the quantities of matter be different, but the temperatures the ſame, the quantities of abſolute heat will be in proportion to the quantities of matter. It now appears, that in the ſame homogeneous bodies, if the temperatures be different, but the quantities of matter the ſame, the form being ſuppoſed to remain unaltered, the quantities of abſolute heat will be in proportion to the temperatures, as meaſured by an equidifferential thermometer, and computed

puted from the point of total privation. Hence if the temperature of a body, eſtimated as above, were to be diminiſhed the one half, or the one third, or in any given proportion, the abſolute heat would be diminiſhed in the ſame proportion.

6. When in conſequence of expoſure to heat, bodies that were previouſly ſolid are converted into fluids, they abſorb a quantity of heat, which is neceſſary to their exiſtence in a fluid form, but which does not increaſe their temperature; on the contrary, when the ſame bodies are re-congealed by expoſure to cold, they part with the heat which in the proceſs of melting they had formerly abſorbed.

This important fact was obſerved by Mr. De Luc, in the years 1755 and 1756, as we learn from his work on the atmoſphere, and from his late publication on meteorology. It appears to have been diſcovered nearly at the ſame time by Dr. Black of Edinburgh, who had no opportunity of becoming acquainted with what Mr. Luc had done, and who taught it publicly in his Chemical Lectures, as early as the year 1757 or 1758. It is proper to add, that Dr. Black was the firſt who compleatly eſtabliſhed this diſcovery by a ſeries of ingenious and deciſive experiments. Theſe experiments have lately been communicated to the public by Mr. De Luc, in his excellent Treatiſe on Meteorology; and as they will throw much light on the ſubject of the following diſſertation, I ſhall lay a brief account of them before the reader *.

If a pound of water at 32, be mixed with an equal quantity of that fluid at 172, by an equidifferential thermometer, the temperature of the mixture will be 102, which is the arithmetical mean between the heat of the warm water and of the cold. But if a pound of ice at 32, be mixed with a pound of water at 172, the temperature of the mixture will be 32. Hence it appears, that ice and water at the freezing point are very differently affected by heat. For in the firſt experiment, a quantity of heat which raiſed the thermometer 70 degrees, paſſed from the warm water into the cold, and by this heat the temperature of the cold water was increaſed 70 degrees. In the laſt experiment, a quantity

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* A very accurate account of the Doctrine of Latent that has been alſo publiſhed by Mr. Wilkie, in the Swediſh Tranſactions. See Acta Suec. 1772, p. 97.

which raiſed the thermometer 140 degrees, paſſed from the warm water into the ice, in conſequence of which the latter was melted, but its ſenſible heat was not increaſed, the temperature of the mixture being 32. Whence it follows, that, in the melting of ice, 140 degrees of heat are abſorbed, which do not produce any effect upon the thermometer, and which, as far as reſpects that inſtrument, may be ſaid to exiſt in the water in an inſenſible ſtate.

The heat which the water abſorbs, when it acquires its fluid form, is again ſeparated from it by congelation, as appears from the following experiment. If a pound of water at 32, be mixed with an equal quantity of ice at 4, nearly $\frac{1}{5}$ of the water will be frozen, and the temperature of the mixture will be 32. In this experiment the ice is raiſed from 4 to the freezing point: it is therefore evident, that by the congelation of nearly $\frac{1}{5}$ of a pound of water, a quantity of heat is evolved, ſufficient to raiſe a pound of ice 30 degrees. It is proper to obſerve, that the quantity of water frozen in this experiment, is ſomewhat leſs than $\frac{1}{5}$ of a pound. Let it be conceived, however, for the ſake of illuſtration,

tration, to be preciſely that quantity. It is manifeſt, that if, by the congelation of $\frac{1}{5}$ of a pound of water, a quantity of heat be detached, ſufficient to raiſe the ice in the above experiment 28 degrees, by the congelation of a pound of water, a quantity of heat would be detached, ſufficient to raiſe it five times 28 degrees. Allowance being made for the fractions, which have been neglected in this illuſtration, and for the different capacities of ice and water, it will appear, that the heat which is extricated by the congelation of water, is preciſely equal to that which is abſorbed by the melting of ice. It is, indeed, perfectly agreeable to the ſimplicity of nature, that the heat which bodies abſorb in conſequence of a change of form, ſhould re-appear when they undergo a contrary change.

Hence we may perceive the reaſon of the following phænomenon, which I believe was firſt obſerved by Dr. Black. If water that is freed from air, and which is perfectly at reſt be expoſed to the atmoſphere when it is colder than 32, it will ſink ſeveral degrees below the freezing point, and at the ſame time will retain its fluid form, no part of it being congealed.

gealed. If it then be ſlightly agitated, a portion of it will ſuddenly become ſolid, and the temperature of the mixture, now conſiſting of ice and water, will riſe to 32.

The reaſon is obvious: water expoſed to a degree of cold below 32, does not freeze without the concurrence of a ſlight agitation. When, therefore, water that had been previouſly cooled below 32, is agitated, a part of it freezes, and the portion that becomes ſolid gives off a quantity of heat, ſufficient to raiſe the whole to the freezing point.

If the quantity of water, and the temperature to which it was reduced, previouſly to its agitation, be known; and if the quantity that is congealed, upon ſhaking it, be determined, the degree of cold neceſſary to freeze the whole may be found.

For the ſudden freezing of a part of the water in this experiment, ariſes from its having been reduced to a lower temperature than 32, prior to its agitation: and ſince effects are proportional to their adequate cauſes, it is manifeſt, that if the temperature of the water be more and more diminiſhed in different experiments, the quantity of ice produced by the agitation, will always be proportional to the previous decrement o the temperature below the freezing point That is, if a decrement of temperature, as 10 below 32, freeze a quantity of the water as 1, a double quantity will be frozen by a double decrement, and ſo in proportion. Let the quantity of water be called Q, let the degree of cold neceſſary to freeze the whole be S, let any other degree of cold leſs than S be d, and let the ice produced by that degree be m. Then $m : d :: Q : S$; and if m, d, and Q, be given, S will alſo be given, for it will be equal to $\frac{d\,Q}{m}$. That is, if the quantity of ice produced as above by any given degree of cold, be determined by experiment, the degree of cold neceſſary to freeze the whole, may be found by calculation. Whence the comparative heats of ice and water will alſo be given: for the comparative heat of water will be to that of ice, as the degree of cold neceſſary to freeze the whole, is to the heat abſorbed by water in the melting of ice.

It has alſo been proved by Dr. Black, that when water is converted into vapour, a quantity of heat is abſorbed, which is neceſſary to the exiſtence of the vapour in the form of an elaſtic fluid, but which does not increaſe its temperature.

This ingenious philoſopher was firſt led to the diſcovery of the abſorption of heat by aqueous vapour, in conſequence of an unexpected appearance which occurred in an experiment made with water at a high temperature. A quantity of that fluid having been raiſed in Papin's digeſter to a temperature many degrees above the boiling point, was ſuffered to communicate with the external air by opening a ſtop cock, upon which a part of it was inſtantly converted into vapour, and the water at the ſame moment ſank to 212. Whence it was concluded, that the vapour produced in the experiment abſorbed a quantity of heat from the water, by means of which the temperature of the latter was ſuddenly reduced to the boiling point. The truth of this concluſion was afterwards fully eſtabliſhed by the ſame philoſopher.

The following experiments appear to prove it deciſively.

If eight pounds of the filings of iron at 212, be mixed with a pound of water at 32, the temperature of the mixture will be nearly

122; the iron will be cooled 90 degrees, an the water heated 90. But if eight pound of iron filings at 300, be mixed with a poun of water at the boiling point, the temperature of the mixture will be 212, and a par of the water will be ſuddenly converted int vapour. If a very ſenſible thermometer b ſuſpended in ſuch a manner as to be in contact with the vapour that is thus produced the latter will alſo be found to have the temperature of 212. In this experiment, 88 degrees of heat are ſeparated from the iron, by which a portion of the water is elevated in the form of vapour, but its ſenſible heat is not increaſed, the mixture having the temperature of 212, and the vapour that eſcapes during the experiment being alſo at the boiling point. Hence, as the heat ſeparated from the iron is not communicated to the water, it muſt neceſſarily be abſorbed by the vapour. In this concluſion we are confirmed by obſerving, that the heat which is thus abſorbed is again ſeparated from the vapour when it is condenſed.

We are informed by Mr. De Luc, in his Treatiſe on Meteorology, that the quantity of heat produced by the condenſation of a given weight of vapour, as determined by the experiments of Mr. Watt, would be ſufficient to raiſe an equal weight of a non-evaporal ſubſtance having the ſame capacity with water, 943 degrees *.

From the heat which is evolved in the freezing of water, we may perceive the reaſon why that fluid, when it is expoſed to a degree of cold lower than 32, and is at the ſame time gently agitated by the wind, ceaſes to cool as ſoon as it arrives at the freezing point, its temperature remaining ſtationary, until the whole is congealed. For it is well known, that 32 is the point at which water, in a ſtate of gentle agitation, becomes ſolid. When, therefore, it is reduced to that point, it begins to freeze, and conſequently to evolve heat.

As the evolution of heat in this inſtance immediately depends upon the congelation, it is manifeſt, that the quantity of heat evolved in a given time, will be in proportion to the quantity of water congealed. But the congelation of the water ariſes from the refrigerating power of the ambient medium;

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* See Idees ſur la Meteorologie, tome premier, p. 224.

the quantity of water congealed will therefore always be proportional to that power. If, for example, by expoſure to a medium 10 degrees below the freezing point, a quantity, as 1, be frozen in a given time, by expoſure to a medium 20 degrees below the freezing point, double that quantity will be congealed in an equal time.

Whence it appears that water, in congealing, is acted upon by two oppoſite powers; it is deprived of heat, by expoſure to a medium which has a lower temperature than 32; and it is ſupplied with heat, by the evolution of that principle in conſequence of the congelation. As theſe powers are preciſely equal to each other, the temperature of the freezing mixture muſt remain unvaried.

In like manner as if water at the ſame moment be permitted to enter into a veſſel by one aperture, and to eſcape from it by another; and if the quantity which eſcapes be equal to that which enters, the height of the water in the veſſel will continue ſtationary.

Hence alſo we may perceive the reaſon why melting ice has a fixed temperature. For ſuch is the nature of this ſubſtance, that it cannot acquire a greater heat than 32, without becoming fluid; and it was proved, that the water which is formed by the melting of ice, abſorbs, at the moment of its formation, a quantity of heat which does not increaſe its temperature.

When, therefore, ice is placed in a medium warmer than 32, as it cannot riſe above that point without melting, the heat which it receives will be wholly employed in changing it into a fluid, and will conſequently be rendered inſenſible.

To the operation of the ſame cauſes it is owing, that the temperature of water boiling, as well as of ſteam during its condenſation, is ſtationary, the abſorption of heat in theſe proceſſes being equal to the ſupply. Hence it may be inferred in general, that if bodies, while they are undergoing a change of form, in conſequence of expoſure to heat or cold, have a fixed temperature, they either abſorb or evolve heat. Moreover, if the temperature remain fixed, while the body is expoſed to a cold medium, we may conclude that it is evolving heat; and if, on the contrary, the temperature be unaltered, while it is expoſed to a warm medium, we may infer that it is abſorbing heat.

Dr. Black has found that many ſubſtances, as tallow, ſpermaceti, bees-wax, roſin, have a fixed temperature, when they are undergoing a change of form, by melting, or by congelation. Whence he infers, that during the former proceſs they abſorb, and during the latter, evolve heat.

From theſe facts and obſervations we may conclude univerſally, that *when bodies, in conſequence of expoſure to heat, have arrived at the melting or boiling points, they abſorb a quantity of heat, which is neceſſary to their exiſtence in the ſtate of non-elaſtic fluids, or of vapour, but which does not increaſe their temperature: and, on the contrary, when vapours are condenſed, or non-elaſtic fluids are congealed, they part with the heat which they had formerly abſorbed.*

The following proportions obtain with regard to the congelation of fluids, and the melting of ſolids, as well as to the elevation and condenſation of vapours.

It is manifeſt, that in the congealing of different portions of the ſame fluid, if the times of the congelation be equal, the quantities congealed will be directly proportional to the power of the cooling cauſe, or to the velocity of the ſeparation of heat. If, for example, a certain quantity of water be conceived to be frozen by a cooling cauſe, which ſeparates the heat with a velocity as 1, it is evident that the velocity of the ſeparation of heat being doubled, a double quantity will be frozen in an equal time.

Let T be the time of the congelation, Q the quantity of matter, and P the power of the cooling cauſe. If T be given, Q will be as P. Again, if the power of the cooling cauſe be given, the quantity congealed will be as the times of the congelation; that is, if P be given, Q will be as T; and if neither be given, Q will be as P×T, conſequently T will be as $\frac{Q}{P}$, and if Q be given, T will be reciprocally as P. That is, if the quantities congealed be given, the times of the congelation will be inverſely proportional to the power of the cooling cauſe; in other words, the time required to freeze a given quantity of the fluid, will be diminiſhed in proportion as the power of the cooling cauſe is increaſed. Hence the congelation will be momentaneous, or the fluid will be entirely changed into a ſolid at the ſame moment, if the power of the cooling cauſe be ſuch as inſtantly to ſeparate the whole of the heat evolved in the congelation. Moreover, if the velocity of the ſeparation of heat be given, the times required to congeal equal quantities of different fluids, will be in proportion to the quantities of heat which they give off in the freezing proceſs. If, for example, the heat given off by water in freezing be to that given off by the congelation of an equal quantity of ſpermaceti, as 2 to 1; and if the velocities of the ſeparation of heat be equal, the time required to change the former into a ſolid will be double that which is required to produce the ſame change in the latter.

Hence the quantities of heat evolved by the freezing of different fluids may be inferred from the times of their congelation, when they are expoſed to cooling cauſes, the powers of which are equal.

It was before proved, that if bodies retain the ſame form, their capacities for heat are not varied by a change of temperature. From the experiments of Dr. Irvine, there is the utmoſt reaſon to believe, that if by a change of temperature the forms of bodies be altered, their capacities for heat are increaſed or diminiſhed, in conſequence of which they muſt neceſſarily abſorb or evolve heat. And thus the celebrated diſcovery recited above, is traced to a general law of nature. I ſhall not, however, enter more particularly into this ſubject at preſent, but ſhall leave the farther illuſtration of it to the learned Philoſophers to whom the honour of the diſcovery is due.

V. Unequal quantities of abſolute heat are required to produce equal alterations of temperature in equal weights of heterogeneous bodies.

Thus if the temperature of a pound of mercury be raiſed one degree, and that of a pound of water one degree, as indicated by an equidifferential thermometer, it will be found that unequal quantities of abſolute heat have been communicated to the water, and to the mercury.

It has been thought by ſome Philoſophers, that the quantities of abſolute heat in bodies are in proportion to their denſities. Boerhaave was of opinion, that heat is equally diffuſed through all bodies, the denſeſt as well as the rareſt, and therefore that the quantities of heat in bodies are in proportion to their bulk.

But it appears from experiments, that the law of the diſtribution of heat throughout the various claſſes of natural bodies, is not according to the ratio either of bulk or denſity.

The firſt attempt to determine by experiment the comparative quantities of abſolute heat in bodies, was made by Fahrenheit, at the deſire of Boerhaave.

The following is a ſhort ſketch of this attempt, nearly in the words of the author:

If you take equal quantities of the ſame fluid, and give them different degrees of heat, and mix them intimately together, the temperature of the mixture will be the arithmetical mean between the heat of the warmer and colder fluid.

If, for example, a pint of boiling water at 212, be mixed with a pint of the ſame fluid at 32, the temperature of the mixture will be 122: the warm water will be cooled 90 degrees, and the cold water heated 90.

But if this experiment be made with water and mercury, in the ſame circumſtances, the effect will be very different.

For if you take equal bulks of mercury and water, and give the water a greater degree of heat than the mercury, the heat of the mixture will always be *greater* than the arithmetical mean.

On the other hand, if the mercury be hotter than the water, the temperature of the mixture will conſtantly be *leſs* than the arithmetical mean. The changes which are produced in the temperature of the water and mercury, in the firſt of theſe inſtances, are found to correſpond to thoſe which are produced, by mixing three parts of hot water with two of cold; and in the ſecond inſtance, to thoſe which take place, when three parts of cold water are mixed with two of hot. That is, the change produced in the heat of the mercury, is to that produced in the heat of the water, as three to two.

From the former of theſe experiments it was juſtly concluded by Boerhaave, that in the ſame body, the diſtribution of fire is in proportion to the bulk or quantity of matter.

But from the experiments with water and mercury, he concluded very unwarrantably, that heat is equally diffuſed through all bodies, the denſeſt as well as the rareſt; and therefore that the quantities of heat in different bodies, are in proportion to their bulks, or to the ſpaces which they occupy.

Theſe experiments have been repeated and varied in the preſent age, and very different concluſions have been drawn from them.

I have already obſerved, that, if a pint of mercury at 100, be mixed with an equal bulk of water at 50, the change produced in the heat of the mercury, will be to that produced in the heat of the water, as three to two; from which it has been inferred, that the abſolute heat of a pint of mercury, is to that of an equal bulk of water, as two to three: or, in other words, that the comparative quantities of their *abſolute* heats are reciprocally proportionable to the changes which are produced in their *ſenſible* heats, when they are mixed together at different temperatures*.

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* This fact has, for ſeveral years, been taught publickly by Dr. Black and Dr. Irvine, in the Univerſities of Edinburgh and Glaſgow: and it has been applied by Dr. Irvine, to the ſolution of a variety of curious and important phenomena.

The truth of this concluſion may be illuſtrated in the following manner.

If four pounds of diaphoretick antimony at 20, be mixed with one pound of ice at 32, the temperature of the mixture will be nearly 26. The ice will be cooled ſix degrees, and the antimony heated ſix. If we reverſe the experiment, the effect will be the ſame. That is, if we take ſix degrees of heat from four pounds of antimony, and add it to a pound of ice, the latter will be heated ſix degrees. The ſame quantity of heat, therefore, which raiſes a pound of ice ſix degrees, will raiſe four pounds of antimony ſix degrees.

If this experiment be made at different temperatures, we ſhall have a ſimilar reſult. If, for example, the antimony at 15, or at any given degree below the freezing point, be mixed with the ice at 32, the heat of the mixture will be the arithmetical mean between that of the warmer and colder ſubſtance. And ſince the capacities of bodies are permanent as long as they retain the ſame form, we infer, that the reſult would be ſimilar, if the antimony were deprived of its whole heat, and were mixed with the ice at 32. But it is evident, that, in this caſe, the ice would communicate to the antimony the half of its abſolute heat. For, if 200 below froſt, be conceived to be the point of total privation, the antimony will be wholly deprived of its heat, when its temperature is diminiſhed 200 degrees below 32; and the heat contained in the ice, when at 32, will be 200 degrees. If we now ſuppoſe them to be mixed together, the temperature of the mixture will be half the exceſs of the hotter above the colder; or the ice will be cooled 100 degrees, and the antimony heated 100. The one half of the heat, therefore, which was contained in the ice, previouſly to the mixture, will be communicated to the antimony: from which it is manifeſt, that, after the mixture, the ice and antimony muſt contain equal quantities of abſolute heat.

To place this in another light, it has been proved, that the ſame quantity of heat which raiſes a pound of ice ſix degrees, will raiſe four pounds of antimony ſix degrees. And as the capacities of bodies, while they retain the ſame form, are not altered by a change of temperature, it follows that the ſame quantity of heat which raiſes the ice 200 degrees, or any given number of degrees, will raiſe the antimony an equal number of degrees.

A pound of ice, therefore, and four pounds of antimony, when at the ſame temperature, contain equal quantities of abſolute heat. But, it appears from the third general fact, that four pounds of antimony contain four times as much abſolute heat, as one pound of antimony; and hence the quantity of abſolute heat in a pound of ice, is to that in a pound of antimony as four to one.

Again, if a pound of ice at 32, be mixed with a pound of antimony at 7, the temperature of the mixture will be 27; the ice will be cooled five degrees, and the antimony heated 20; or the change produced in the ſenſible heat of the ice, will be to that produced in the ſenſible heat of the antimony, as one to four. But it was before proved, that the abſolute heat of a pound of ice, is to that of a pound of antimony, as four to one. From which it is evident, that the comparative quantities of abſolute heat, in equal weights of ice and antimony, having the ſame temperature, are reciprocally proportionable to the changes produced in their ſenſible heats, when they are mixed together at different temperatures *.

This concluſion appears to be juſtly deduced from the premiſes. The reaſoning which has been employed to eſtabliſh it, is founded upon the neceſſary proportion and connection between cauſes and effects, and is altogether independent of any hypotheſis reſpecting the nature of heat.

It may be conſidered as a particular application of the following univerſal theorem. If equal cauſes, operating upon different claſſes of bodies, produce unequal effects, in order to the production of equal effects, unequal cauſes will be required: in other words, if the effects vary when the cauſes are equal, the cauſes will vary when the effects are equal;

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* I have thus endeavoured briefly to eſtabliſh the truth of the above doctrine, as a neceſſary introduction to the experiments which follow; but the more full and complete illuſtration of it I ſhall leave to Dr. Black and Dr. Irvine.

This diſcovery opens a wide field for inveſtigation, as by means of it we are enabled to eſtimate the comparative quantities of abſolute heat in bodies, and to determine with certainty and accuracy, the various proportions in which the element of fire is diſtributed throughout the different kingdoms of Nature.

equal; and if, in each claſs of bodies, the effect be proportional to its cauſe, the variations of cauſes and effects, in the different claſſes, will be reciprocally proportional to each other. The proof is the ſame with that of the foregoing propoſition, ſubſtituting the term *effect* for temperature, and the term *cauſe* for abſolute heat.

This reaſoning will apply not only to the inveſtigation of the comparative heats of bodies, but alſo to all other branches of knowledge in which the above-mentioned reciprocation of cauſes and effects takes place. It may be extended, for example, to menſtruums and ſolvends in chymiſtry, to forces and the motions produced by them, in natural philoſophy, as well as to the diminutions of forces, and the powers by which they are diminiſhed. Thus let A and B be two bodies, and let C be a moving force; let this force be ſuch, that the double, treble, quadruple, or $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, of it may be taken. The quantities of the moving force required to produce equal velocities in A and B, will be reciprocally as the velocities communicated to thoſe bodies when the moving forces are equal. This neceſſarily follows from what has been demonſtrated by natural Philoſophers reſpecting forces, and the velocities which they communicate. For let equal forces be applied to A and B, and let the velocity of the former be to that of the latter, as 2 to 1; ſince, by the ſecond law of nature, in the motions communicated to the ſame body, the forces are as the velocities, if the velocity of A be conceived to be diminiſhed the one half, or to become equal to that of B, the force required to produce this velocity will be diminiſhed in the ſame proportion; conſequently when A and B move with equal velocities, the force applied to the former will be half of that applied to the latter. Hence it appears, that if with equal moving forces, the velocities be as 2 to 1, with equal velocities, the moving forces will be as 1 to 2. The ſame reaſoning will apply to menſtruums and ſolvends.

Thus let A and B be two menſtruums, and let C be a ſubſtance to be diſſolved in them; let the degrees, by which A and B approximate to ſaturation by given additions of the ſolvend, be ſuppoſed to be known. If we find in the ſeveral intermediate degrees, between the points of ſaturation and of total privation, that the ſame quantity of the ſolvend which makes A approximate one degree towards ſaturation, will make B approximate two degrees, we may conclude that the diſſolving power of the former is to that of the latter, as 2 to 1; and if A and B be both ſaturated, or have made equal approaches towards ſaturation, the quantity of the ſolvend contained in A will be double that contained in B.

Thus it appears, that the comparative quantities of abſolute heat in bodies may be determined, by mixing them together as above, and obſerving the changes which are produced in their ſenſible heats. This rule, however, does not apply to thoſe ſubſtances, which, in mixture, excite ſenſible heat or cold by chemical action.

SECTION II.

Having premiſed theſe general facts, I ſhall now lay before the reader my Experiments on Animal Heat, and the Inflammation of Combuſtible Bodies.

It was obſerved above, that ſenſible heat has a conſtant tendency to diffuſe itſelf equally ly over all bodies, till they are brought to the ſame temperature. From this property of heat, it is manifeſt, that thoſe animals which have a higher temperature than the medium in which they live, muſt be continually communicating heat to the ſurrounding bodies. Since therefore, in the animal kingdom, there is a conſtant diſſipation of heat, it follows, that there muſt be a proportionable ſupply of this element, to repair the waſte. For, if the animal body had not the power of exciting or collecting heat, it would ſoon arrive at the temperature of the ambient medium.

With a view to diſcover the nature of this power, I made a variety of experiments, in the ſummer 1777, on animal, vegetable and mineral ſubſtances; ſome of which I ſhall now relate, as I think they have led to the true ſource, from whence the heat of animals, and the heat which is produced by the inflammation of combuſtible bodies, is derived.

I muſt firſt obſerve, that experiments for determining the comparative quantities of heat in bodies, by mixing them together at different temperatures, are liable to ſeveral cauſes of inaccuracy. 1. When the ſubſtance ſtances to be compared, are mixed together, a certain time is required for the heat to paſs from the warmer to the colder body, till they arrive at the ſame temperature. If they be *intimately* mixed, and the veſſel be a little agitated, a minute is generally ſufficient. During this interval, a part of the heat is carried off by the ſurrounding atmoſphere. It therefore becomes neceſſary, to calculate the heat which is thus loſt in the firſt minute.

It was formerly remarked, that when a heated body is placed in a cold medium, the decrements of heat are partly equable, and partly in geometrical progreſſion. If the body tranſmit heat very faſt, and its temperature be much greater than that of the ambient medium, the decrements are nearly in geometrical progreſſion, agreeably to the rule laid down by Sir Iſaac Newton; the error produced by calculating according to this rule being ſo inconſiderable that it may be neglected.

On the contrary, when the experiment is made in a veſſel that tranſmits heat very ſlowly, and the heat of the ſubſtance to be examined is not much greater than that of the atmoſphere, the quantities of heat loſt in any two ſmall ſucceſſive portions of time, approach ſo nearly to an equality, that the difference cannot be diſtinguiſhed by the niceſt thermometer. That this obſervation is well founded, appears from the following experiment:

A pound of water in an earthen veſſel, being raiſed to 120, the temperature at the end of

1	minute was	—	119
2	—	—	118
3	—	—	117
4	—	—	116
5	—	—	115
6	—	—	114
7	—	—	113

In the experiments which I ſhall hereafter relate, the order of cooling was obſerved for ſeveral minutes. When the decrements of heat were found to be ſenſibly different from each other, the heat loſt in the firſt minute was calculated according to Sir Iſaac Newton's rule, from the ſeries of numbers determined by obſervation.

But in general the experiments were made under ſuch circumſtances, that the heat paſſed off very ſlowly; in which caſe the difference between the decrements is ſo ſmall, as not to be diſtinguiſhed by the eye. And, therefore, the heat loſt in the firſt minute may, without any ſenſible error, be conſidered as equal to that loſt in thoſe which immediately follow.

I muſt here obſerve, that I believe Dr. Irvine was the firſt who applied the abovementioned rule of Sir Iſaac Newton, to calculate the heat loſt during the firſt minute, in experiments for determining the comparative quantities of heat in bodies.

2. If the experiment be made by adding the warmer ſubſtance to the colder, the former will loſe a part of its heat in paſſing through the air. To avoid this cauſe of inaccuracy, the experiment ſhould be reverſed: the colder ſubſtance ſhould be taken at the temperature of the air in the room, and in that ſtate being mixed with the warmer, it cannot, during its paſſage through the air, loſe any part of its heat.

3. If the ſubſtance, which has the greateſt heat, be mixed with that which has the leaſt in, a cold veſſel, a part of the heat will be communicated to the veſſel. But if the ex-

periment be made in a warm veſſel, the cold ſubſtance will receive heat, not only from the warm ſubſtance, but from the veſſel in which it is contained.

The moſt effectual method of avoiding this cauſe of inaccuracy, is firſt to determine the capacity of the veſſel for receiving heat, compared with that of one of the ſubſtances to be examined; and the capacity of the veſſel being known, its influence upon the temperature of the mixture may be found. Let a pound of water, for example, be one of the ſubſtances, whoſe comparative heat is to be examined; and let the capacity of the water be to that of the veſſel as 12 to 1. If in any experiment we find that the veſſel has received 12 degrees of heat from a pound of water, we may be ſure that the ſeparation of this heat has cooled the water one degree.

4. In experiments for determining the comparative quantities of heat in bodies, water is generally the ſtandard. When the body, which is to have its heat compared with that of water, tranſmits heat very ſlowly, it frequently happens, that the different parts of the mixture cannot be brought preciſely to the ſame temperature for ſeveral

minutes. Thus I find that in experiments upon vegetables and the calces of metals, there is a conſiderable difference, at the end of the firſt minute, between the heat of the mixture at the ſurface, and that at the bottom of the veſſel; ariſing partly from the tendency of the warmeſt part of the water to remain at the ſurface, and partly from the difficulty with which the above-mentioned ſubſtances tranſmit heat. This may be remedied in ſome degree, by mixing the bodies together intimately, and agitating the mixture briſkly. But by much agitation, the heat is carried off ſuddenly and irregularly, which makes it difficult to calculate the heat that is loſt in the firſt minute.

To remedy this cauſe of error, a moderate agitation ſhould be uſed, and the experiment ſhould be made, in ſuch circumſtances, that the mixture ſhall cool very ſlowly, in order that as ſmall a portion of the heat as poſſible may eſcape, while its parts are acquiring a common temperature.

The mixture may be made to cool ſlowly, if the ſubſtances, that are to be mixed together, be taken in conſiderable quantity, and if the veſſel in which the mixture is contained be ſurrounded on all ſides with a covering that ſlowly tranſmits heat.

It is moreover evident, that the mixture will cool ſlowly if its temperature do not much exceed that of the external air. The exceſs of the temperature of the mixture above that of the ſurrounding air, all other circumſtanees being equal, will be in proportion to the difference between the temperature of the colder and warmer ſubſtance, the former being ſuppoſed, previouſly to the experiment, to have the ſame heat with the ambient medium. It is, therefore, proper, with a view to diminiſh the velocity of the refrigeration, that the warmer ſubſtance ſhould not be raiſed to a very high temperature. On the contrary, if the difference between the heat of the warmer and colder ſubſtance be very ſmall, thoſe ſubſtances will ſuffer but little alterations in their temperatures when they are mixed together. But theſe alterations are the meaſures of the capacities; and if the meaſures of the capacities be greatly diminiſhed, it is manifeſt that the unavoidable errors, which are produced by the imperfections of the ſenſes, and of thermometers, will render the reſults of the experiments,

ments, and the calculations which are deduced from them, precarious.

Hence it follows, that in trials made with a view to determine the comparative heats of bodies by mixture, very ſmall as well as very great differences of temperature ſhould be avoided. There is, however, a certain limit between theſe extremes which will be leaſt liable to error; and this limit is to be determined by varying the difference between the temperatures of the colder and warmer ſubſtance in different experiments, and obſerving thoſe reſults which moſt nearly coincide with each other. If accurate thermometers be uſed, and if the other ſources of fallacy be carefully avoided, the reſults of the trials, made with conſiderable variations of temperature, will be nearly the ſame, as appears from the following experiments:

EXPERIMENT I.

Air in the room —	65
Thirty-ſix ounces troy of water, at	123
were introduced as ſpeedily as poſſible into an earthen veſſel at	65.2

The temperature of the water was meaſured by a thermometer, which had the mercury contained in a ſmall cylinder, that reached from the ſurface to the bottom.

At the end of

1	minute it was	—	116.2	
2	—	—	115.1	
3	—	—	114.5	
4	—	—	114	minus.
5	—	—	113.5	
6	—	—	113	

The veſſel in which this experiment was made weighed 30 ounces and $\frac{3}{4}$ troy. It was three inches and $\frac{5}{8}$ in diameter, and nine inches in depth.

From the experiment it appears, that the water loſt in the courſe of the ſecond minute a little more than a degree, and in the four ſucceeding minutes it cooled very nearly at the rate of half a degree in a minute. Hence it follows, that the water and the veſſel had not acquired a common temperature till the ſecond minute was expired, the earthy matter of which the veſſel was compoſed being a bad conductor of heat, and its thickneſs being nearly one fourth of an inch. As the heat loſt, when the progreſs of cooling became regular, was half a degree during each minute, we may ſafely conclude, that the quantity of heat carried off by the air, in the firſt and ſecond minutes, was nearly one degree. Adding therefore one degree to 115.1, which was the temperature of the water at the end of the ſecond minute, we have 116.1 for the true temperature of the water and veſſel. Subtracting this from 123 we have 6.9 for the remainder. The water was therefore cooled by the veſſel 6.9. The veſſel was raiſed from 65.2 to 116.1 or 50.9; and ſince it received this heat from the water, it is evident that the ſame heat which changes the temperature of 32 ounces of water 6.9, will change the temperature of the veſſel 50.9. And, by a parity of reaſoning, the ſame heat which raiſes the water one degree, will raiſe the veſſel 7.37. The capacity of the water is conſequently to that of the veſſel as 7.37 to 1.

EXPERIMENT II.

Air in the room	—	65
Thirty-two ounces of water at	—	151.75
were poured into the earthen veſſel at	—	65.25

The temperature of the water in

1	minute was		—	141 plus
2	minutes		—	139.75
3	—		—	138.5
4	—		—	137.5
5	—		—	136.5
6	—		—	135.5
7	—		—	134.5 plus
8	—		—	133.7 plus

After the third minute was expired, the progreſs of cooling was nearly one degree in a minute. If, therefore, we allow three degrees for the heat carried off by the air during the firſt three minutes, we ſhall have 141.5 for the true temperature.

Hence it appears, that the water was cooled by the veſſel 10.25, and the veſſel heated by the water 76.25. By this experiment the capacity of the water was to that of the veſſel as 7.33 to 1.

EXPERIMENT III.

Air in the room	—	66.5
Thirty-two ounces of water at		179
were poured into the earthen veſſel at	— —	66.5

In

In 1 min.	the temperature	was	164.4	
2	—	—	162.2	
3	—	—	160.6	
4	—	—	159	
5	—	—	157.5	
6	—	—	156	plus
7	—	—	154.6	

After the third minute was expired, the progreſs of cooling was nearly 1.5 during each minute. If we allow 4.5 degrees for the heat loſt in the firſt three minutes, we ſhall have 165.1 for the true temperature. The water was conſequently cooled by the veſſel 13.9, and the veſſel heated by the water 98.6.

By this experiment, therefore, the capacity of the water is to that of the veſſel as 7.09 to 1. Hence it follows, that when the difference between the temperature of the water and veſſel previouſly to the experiment is increaſed, the capacity of the former compared with that of the latter, is apparently diminiſhed. The reaſon is obvious: in proportion as the water is raiſed to a higher temperature, more heat eſcapes during its paſſage through the air; and if allowance be not made for this difference, a greater quantity of heat will appear to be ſeparated by the veſſel. It is manifeſt, however, from the near coincidence of the reſults of the two firſt experiments, that the fallacy ariſing from this cauſe, when the water is not raiſed to a high temperature, is inconſiderable.

It was before obſerved, that the mixture will cool ſlowly if its temperature do not much exceed that of the external air. It may be proper to add, that if the colder ſubſtance be taken at a lower temperature than the air in the room, and the warmer ſubſtance at a higher; and if the difference of their heats be ſo proportioned, that the common temperature, after they are mixed together, ſhall be nearly the ſame with that of the air in the room; the fallacy, ariſing from the refrigeration of the mixture, will be altogether avoided, at the ſame time that a large ſcale may be obtained.

5. The volumes of the ſubſtances, which are to have their heats compared, ſhould be as nearly equal as poſſible. For if the volumes be unequal, as the ſeveral parts of the warmer and colder ſubſtances cannot be brought into immediate contact with each other, the difficulty of diffuſing the heat uniformly over the mixture will be increaſed.

Let water, for example, and ſand, be the ſubſtances which are to have their comparative heats examined. If the volume of the ſand be leſs than that of the water, the depth of the latter, when they are mixed together, will exceed that of the former; and if it be greater, the reverſe of this will take place. In both caſes it will become more difficult to bring the ſand and water to a common temperature.

If the volume of the water exceed that of the ſand, and if, in repeating the experiment, the comparative bulks of theſe ſubſtances be varied, an equal degree of agitation being given to the mixture, the capacity of the water, compared with that of the ſand, as determined by a thermometer which reaches from the ſurface to the bottom, will appear to diminiſh when the volume of the ſand is diminiſhed. This is proved by the following experiments:

EXPERIMENT I.

Air in the room —	49
Thirty-eight ounces of common ſand at — —	53½
	Mixed

Mixed with 16½ ounces of water at — —	149
The mixture being agitated for a ſhort time, its temperature in 1 minute was —	117½
2 — —	117½
3 — —	117¼
4 — —	116¾
5 — —	116¼
6 — —	115¾
7 — —	115½
8 — —	115 plus
9 — —	114¾
10 — —	114¼ plus

EXPERIMENT II.

Air in the room —	61.5
Twenty-two ounces of common ſand at —	62¾
Mixed with 22 ounces of water at —	143 plus
The mixture being agitated as before, its temperature in 1 minute was —	128 minus
2 — —	127½ minus
3 — —	127 minus

4 minutes

4 minutes was		—	126¼
5	—	—	125¼
6	—	—	124¼
7	—	—	123½
8	—	—	122¾
9	—	—	122
10	—	—	121¼
11	—	—	120½
12	—	—	120
13	—	—	119½

The agitation given to the mixture in both experiments was nearly equal. I found from ſubſequent trials that the heat imparted to the air by the agitation was nearly one degree. The experiments were made in the ſame veſſel, the heat of the mixture was meaſured by cylindrical thermometers, that reached from the ſurface to the bottom, a ſmaller thermometer being uſed for the firſt experiment than for the ſecond. Allowing one degree for the heat loſt during the firſt minute, the capacity of water will be to that of the ſand, by the firſt experiment, as 5 to 1; and by the ſecond as 4.7 to 1. Thus it appears, that in the foregoing experiments with ſand and water, the capacity of the latter was apparently diminiſhed, when the comparative bulk of the former was diminiſhed.

This fallacy ariſes from the increaſed difficulty of bringing the water and ſand to a common temperature. For as the capacity of water is greater than that of ſand, when theſe ſubſtances are mixed together, the common heat of the mixture will be nearer to the original temperature of the water than to that of the ſand. If the ſand and water be not brought to a common temperature, in other words, if the temperature of the water at the ſurface exceed that of the ſand at the bottom, a cylindrical thermometer, the length of which is equal to the depth of the mixture, will give a degree of heat that is intermediate between the temperature at the ſurface at the bottom.

But this intermediate temperature will be leſs than that which would be produced, by bringing the ſand and water to a common heat; becauſe as the capacities are different, if the parts of the mixture were reduced to a common heat, the increaſe in the temperature of the ſand would be greater than the diminution in that of the water.

Hence the water will be ſeemingly more refrigerated, and the ſand leſs heated; and conſequently the relative capacity of the former will appear to diminiſh, and that of the latter to increaſe, in proportion as the heat is more unequally diffuſed over the mixture; and ſince the inequality in the bulk of the ſubſtances compared contributes to a more unequal diffuſion of the heat, it follows, that an increaſe in the exceſs of the volume of the water above that of the ſand, all other circumſtances being equal, muſt be accompanied with an apparent diminution in the relative capacity of the water. Hence, by taking volumes perfectly equal, we ſhall have the limit at which the relative capacity of water will appear to be the greateſt, and which will conſequently give us a reſult that will approach neareſt to the truth. For in that caſe the above-mentioned cauſe of fallacy, tending to make the *apparent* proportion of the capacity of water to that of ſand leſs than the *real* proportion, will be moſt effectually avoided.

From the foregoing obſervations it follows, that, in experiments for determining the comparative heats of bodies by mixing them together at different temperatures, if all other circumſtances be equal, the comparative heat of that which has the greater capacity will appear to diminiſh in proportion as the heat is more unequally diffuſed over the mixture. And as ſolid bodies have leſs capacities for heat than water, it likewiſe appears, that in experiments for aſcertaining the capacities of ſuch bodies by mixing them with this fluid, if the latter have the greater volume, the relative capacity of the water, as determined by a cylindrical thermometer which reaches from the ſurface to the bottom of the mixture, will be apparently diminiſhed by an unequal diſtribution of the heat.

It is manifeſt, that, in all caſes, the facility of promoting an uniform diffuſion of the heat will be increaſed, by bringing the parts of the colder and warmer ſubſtances cloſely into contact with each other, which will be moſt eaſily accompliſhed by mixing them in equal volumes, and by ſubdividing the ſolids into minute parts.

If the cauſes of fallacy, now explained, be carefully avoided; if the ſubſtances, which are to have their heats examined, be uſed in ſuch quantities, and placed in ſuch a ſituation, after they are mixed together, that they ſhall cool very ſlowly; if, at the ſame time, the difference of their temperatures be ſuch as to afford a ſcale ſufficiently large; if the heat be uniformly diffuſed over the mixture, and its temperature exactly aſcertained, the reſults of different experiments will nearly coincide with each other, and the comparative heats will be determined with a degree of accuracy which will approach near to the truth.

It is proper to add, that though conſiderable variations ſhould occur in the reſults of different experiments, yet the general concluſions deduced from them may, in many caſes, be as certain as if they admitted of perfect preciſion. Thus if the capacity of the calx of iron, for receiving and communicating heat, be compared with that of iron; and if we find in a variety of trials that the proportion of the former to the latter is never greater than 4½ to 1, and never leſs than 3½ to 1, we may conclude with the utmoſt certainty, that the capacity of the calx conſiderably exceeds that of the metal; and moreover, if the experiments have been made with proper care, and have been frequently repeated, we ſhall have good evidence for believing that the exact ratio of the capacities of theſe ſubſtances is in ſome of the intermediate points between the extremes; that is, between the proportions of 3½ to 1, and of 4½ to 1. If, however, as was before remarked, the above-mentioned ſources of fallacy be obſerved with attention, and carefully avoided, we ſhall have a near coincidence in the reſults of different trials.

With theſe precautions the following experiments were made to determine the comparative heats of ſome of the moſt common vegetable and animal ſubſtances, water being uſed as a ſtandard.

EXPERIMENT I.

Air in the room	—	64	
Twenty-two ounces of dried wheat	—	63.7	plus
Mixed with 20 ounces of water at	—	120	
Temperature of the mixture in			
2 minutes was	—	101.7	plus
3 —	—	101.7	
4 —	—	101.6	

5 minutes		—	101.6
6	—	—	101.5
7	—	—	101.5
8	—	—	101.5 minus
9	—	—	101.5 minus
10	—	—	101.4 minus
11	—	—	101.3
12	—	—	101.3
13	—	—	101.2 plus
14	—	—	101.2
16	—	—	101

The parts of the mixture being brought by agitation to a common temperature, the mean heat at the end of the 22d minute was 98.5.

The mixture was agitated during the firſt minute by means of a rod 12 times: its temperature was meaſured by a thermometer which reached from the ſurface to the bottom: the quantities of wheat and water were ſo proportioned that the volumes ſhould be nearly equal. The cold wheat was poured into the veſſel containing the warm water. The veſſel was placed on a ſlip of flannel, and its mouth was covered with flannel, to prevent the acceſs of the air in the courſe of the experiment.

It appears, that the mixture from the end of the ſecond to that of the ninth minute loſt three-tenths of a degree, and from the end of the ninth to that of the ſixteenth, it loſt ſix-tenths of a degree. The velocity of the refrigeration was therefore accelerated: for which the following cauſes may be aſſigned: when a body, that tranſmits heat ſlowly is expoſed to a medium colder than itſelf, it cools more quickly at the ſurface than at the center; in conſequence of which, a difference of temperature will ariſe in thoſe parts of the mixture. This difference will, for a certain length of time, continue to increaſe, and during that time the progreſs of cooling at the center will be accelerated. For it is manifeſt, that, in the cooling proceſs, the particles at the ſurface are continually receiving heat from thoſe at the center, and giving off heat to the external air. During the firſt moments of the refrigeration, as there is but little difference between the temperature of the particles at the center and of thoſe at the ſurface, the tranſmiſſion of the heat from the former to the latter is very ſlow. But the particles at the ſurface will at this time cool moſt ſpeedily, becauſe the difference between their temperature, and that of the ſurrounding air, is greater than at any other period of the refrigeration. Since, therefore, the particles at the ſurface, in the beginning of the cooling proceſs, receive heat ſlowly from the center, and are, at the ſame time, quickly deprived of heat by the external air; the ſeparation of that principle from thoſe particles, will, during this part of the proceſs, exceed the ſupply. The difference between the temperature at the center and at the ſurface will conſequently increaſe, and the velocity of the refrigeration at the center will be accelerated. But the rate of cooling at the ſurface is retarded, in proportion as the body approaches more nearly to the temperature of the ambient medium; and this retardation will gradually operate to diminiſh the rate of cooling at the center.

Hence, in the foregoing experiment, the refrigeration at the center was ſlow at firſt, and was gradually accelerated for a certain length of time, till it arrived at a maximum, after which it began to be retarded.

From the difference between the heat at the ſurface and at the center, we may account for the ſudden diminution of temperature when the mixture was agitated at the end of the 16th minute. For the thermometer, previouſly to the agitation, indicated the heat at the center which was the warmeſt part of the mixture. And, therefore, when the ſeveral parts were brought nearly to a common heat, the thermometer ſank 2½ degrees.

It will hereafter appear, that when the parts of the mixture were previouſly brought to a temperature perfectly uniform, it loſt by an agitation equal to that given to it, in theſe inſtances, half a degree. Allowing, therefore, half a degree for the heat ſeparated by the agitation in the courſe of the 22d minute, we ſhall have 99 for the mean temperature at the end of the experiment; but the mean heat at the termination of the 2d minute was 101.7, hence the heat imparted to the air in 21 minutes was 2.7. If we ſuppoſe the mean rate of cooling during that time to be uniform, a ſuppoſition, which as the refrigeration was very ſlow, will be nearly accurate, we ſhall have .12 for the heat loſt in each minute.

Conſequently we have 101.94 for the mean heat at the end of the firſt minute; and if half a degree be added on account of the heat loſt by the agitation at the beginning of the proceſs, the true temperature will be 102.44.

The original heat of the wheat was	63.7
That of the water —	120
The wheat was therefore raiſed	38.74
And the water cooled —	17.56

In this experiment the cold wheat was introduced into the veſſel containing the warm water. It will be proved in the ſequel, that the heat communicated by the veſſel was equal to that which would have been communicated by .84 of an ounce of water.

It appears, therefore, that the wheat cooled 20 ounces of water, and the veſſel which was equal to the .84 of an ounce, 17.56 degrees. From which it follows, that it would have cooled 20.84 ounces of water, 17.56 degrees. The quantity of wheat was 22 ounces. Hence, according to a rule which was firſt propoſed by Dr. Irvine, and which will be more particularly explained hereafter, the comparative heat of water is to that of wheat, in the compound ratio of 38.74 to 17.56, and of 22 to 20.84, or as 2.3 to 1.

I next endeavoured to determine the capacity of the veſſel uſed in the foregoing experiment, compared with that of water.

In order to avoid the inaccuracy ariſing from the heat loſt by pouring the warm water into the cold veſſel, the greater heat was given to the latter, which was raiſed to a fixed temperature by the following contrivance. It was before obſerved, that if the vapour of boiling water be made to flow in a conſtant ſtream through a pipe of any length or dimenſions, every part of that pipe will be raiſed to the boiling point. It is, therefore, evident, that if a ſmall ſpiral pipe be made to wind round a large cylindrical veſſel at a little diſtance from it, and be kept at 212 by a ſtream of aqueous vapour, the veſſel will be gradually heated, till at length it arrive at a certain point below 212, where it will continue without variation as long as the vapour paſſes through the pipe, and the temperature of the air in the room remains unchanged. In figure 4, plate I, the reader will ſee a ſketch of an apparatus contrived on this principle. A B C D repreſents a tinned veſſel nine inches deep, and 5½ inches wide, environed by the ſpiral pipe A E B, placed at the diſtance of a quarter of an inch from it. This pipe having performed one compleat revolution around the veſſel, terminates at the point B, where the ſteam is ſuffered to eſcape. Into the ſpiral pipe, the tube H G is inſerted by means of a cork at one of its extremities G, and at the other extremity H, it is fixed to the boiler H K. In the tube H G is inſerted the cock R, for the purpoſe of occaſionally interrupting the communication between the boiler and the ſpiral pipe, and a cock is fixed to the boiler at P, through which a part of the ſteam is ſuffered to eſcape, when neceſſary. To the ſpiral pipe, at the point *o*, is joined the crooked pipe *o u b*, and to this is joined another *b v y*, that is connected with the circular pipe *a d*, placed at a little diſtance from the bottom of the veſſel A B C D. The perpendicular portion *u b* of the crooked pipe *o u b*, is of ſuch a ſize as to be capable of ſlipping into the correſponding pipe *b v*, the former being adapted to the latter in ſuch a manner, that it may be introduced into it to a greater or to a leſs depth. And hence the circular pipe *a d* may be made to approach towards the bottom of the veſſel, or to recede from it at pleaſure.

The ſuperior ſurface of the veſſel is covered by a lid. This is heated by the ſmall ſpiral pipe *h e*, which is joined at the point *h* to the cock F by a moveable joint, ſimilar to that deſcribed above. The cock is ſoldered to the pipe F G. Hence the ſpiral *e h* being moveable upon the extremity of the cock, may likewiſe be made to approach towards the lid, or to recede from it. It is manifeſt, that by this contrivance the heat communicated at the ſuperior ſurface, and at the bottom of the veſſel may be ſo adjuſted, as to be preciſely equal to that communicated at the lateral ſurface. The paſſage of the ſteam, through the pipe *h e*, may be occaſionally interrupted by turning the ſtop-cock F; and the pipe may be ſeparated from the cock, when it is neceſſary to remove the lid of the veſſel, in order to introduce the fluid or ſolid ſubſtance, which is to be the ſubject of the experiment.

h k l m repreſents a ſimilar veſſel, (fig. 5.) ſurrounded by three ſpiral pipes *a c*, *d f*, *e g*. The middle and inferior *d f*, *e g*, having performed five revolutions each in contact with the cylindrical ſuperficies of the veſſel, and one around the bottom, terminate near to the center.

The ſuperior pipe *a c* being made to croſs the other two twice, revolves only three times around the cylindrical ſurface, after which having made a circular revolution around the bottom, it alſo terminates near to the center.

Into the middle and inferior pipes, which are ſeparated from each other nearly the one-fourth of an inch, are inſerted the ſtop-cocks *o* and *n*, in conſequence of which the ſteam, that is admitted by the conical veſſel *a b*, may be made to paſs either through the ſuperior pipe alone, or through the ſuperior and inferior, or through the three at pleaſure. It is plain, that by this contrivance, three different degrees of heat may be communicated to the veſſel *h k l m*, each of which will be nearly fixed, being liable to be varied only by a change in the temperature of the air in the room.

The veſſel which was to be the ſubject of the experiment, and which was made of tinned iron, was 7⅝ inches deep, 4½ wide, and weighed exactly 10 ounces and two ſcruples troy. It was placed upon a ſlip of flannel in the veſſel *h k l m*, (fig. 5.) which may be called a ſteam bath; the extremity of the latter veſſel being cloſed by a cork that was made to fit it accurately. In this cork a thermometer was fixed that had the degrees marked upon the tube, and which reached to ſuch a depth as to be in contact with the bottom of the interior veſſel.

The ſteam was then made to paſs through the ſuperior ſpiral pipe, in conſequence of which the thermometer gradually roſe to 138.5, where it remained with very little variation.

The air in the room being -	64
A quantity of water, weighing 40 ounces troy, was taken at -	64.5

Its temperature being examined by a very accurate thermometer, that had each degree divided into 10 equal parts, diſtinctly viſible to the naked eye.

The interior veſſel was then ſuddenly removed from the ſteam bath; the water was poured into it, and was agitated for a ſhort time, after which its temperature was examined by the ſame thermometer that had been employed to determine its heat previouſly to the experiment, and was found to be as follows:

1 minute -	65.8
2 -	65.9
5 -	65.9½ very nearly 66.

During the courſe of this experiment, the water was kept in a ſtate of gentle agitation, that all its parts might have an equal temperature. It is manifeſt, that a ſmall quantity of heat muſt have been loſt by the veſſel in the ſhort ſpace of time which was employed to remove it from the ſteam bath. The heat that was thus loſt may be nearly eſtimated by the experiment which follows:

Air in the room -	68¼	nearly
Forty ounces of water	68.15	very nearly
The veſſel was placed in the ſteam bath A B C D, (fig. 4.) and raiſed to	109.25	
The water being poured into the veſſel, its temperature in		
1 minute was -	68.9	
2 -	68.9	plus
3 -	69	minus
4 -	69	minus
7 -	69	

Hence we have 1.5 for the heat communicated to the water in the firſt experiment, and .85 for that communicated in the ſecond. In conſequence of which the comparative heat of the water will be to that of the veſſel by the former experiment as 48 to 1, and by the latter as 47.3 to 1. In the latter caſe, therefore, the ratio of the heat of the water to that of the veſſel appears to be leſs than in the former. The reaſon is obvious: when the veſſel is removed from the ſteam bath, for the purpoſe of receiving the cold water, it muſt neceſſarily loſe more heat in proportion as it was previouſly raiſed to a higher temperature. It is manifeſt, however, from the near coincidence of the reſults of the two experiments, that the quantity of heat loſt in either caſe muſt have been very ſmall. This experiment was frequently repeated, different degrees of heat being given to the veſſel previouſly to the introduction of the water. The following are the reſults of four of the moſt accurate trials.

Comparative heat of water to that of the veſſel by the

1ſt trial as	48 to 1
2	47.4 to 1
3	47.3 to 1
4	47 to 1

From the mean reſult of theſe trials it appears, that the comparative heat of the veſſel was to that of the water as 1 to 47.4.

The quantity of heat therefore contained in the veſſel was equal to that contained in .84 of an ounce of water.

EXPERIMENT II.

Air in the room — 52
Twenty-two ounces of dried wheat at — 53.5
Mixed with 20 ounces of water at — 126.75

the mixture being agitated briſkly and uniformly, with an iron rod, 12 times; and its temperature being meaſured by the ſame thermometer uſed in the preceding experiment, in

1 minute it was — 101
2 — — 101
3 — — 101

The mixture being again agitated 12 times, its temperature at the end of the

4th minute was — 100½
5 — — 100½ minus
9 — — 100

Being again agitated 12 times, at the end of the 10th minute it was		99½
12	— —	99½ minus

It is proper to obſerve, that in agitating the mixture, a motion as nearly equal as poſſible ſhould be communicated to its ſeveral parts. If this circumſtance be not attended to, a variation will be produced in the reſults of different trials.

In this experiment the cold wheat, as before, was poured into the veſſel containing the warm water. The veſſel reſted on a wooden table, which was covered with flannel. Its mouth was cloſed with a cork, to prevent the acceſs of the external air, and the thermometer was introduced into the mixture through an aperture in the cork.

It appears, that when the mixture was agitated at the end of the 3d minute, it loſt half a degree. The ſame thing happened when it was again agitated at the end of the 9th minute. Hence we may infer, that a briſk agitation, continued 12 times, was ſufficient to bring the mixture to a common heat. For it will be proved by a ſubſequent experiment that if this were not the caſe, more heat would have been apparently loſt in the ſecond repetition of the agitation than in the third. As the agitation given to the mixture, in theſe inſtances, was equal to that which it had ſuffered at the beginning of the experiment, it follows that we may allow very nearly half a degree for the heat loſt in the courſe of the firſt minute; for though the heat, ſeparated by the agitation at the beginning, muſt have been a little greater than that which was afterwards ſeparated; yet as the heats loſt at the end of the 3d and 9th minutes were ſo nearly equal, that the difference could not be diſtinguiſhed by the eye, we may ſafely conclude, that the difference between thoſe loſt at the end of the 3d and 1ſt minutes, was ſo ſmall that it may be neglected. Allowing, therefore, half a degree for the heat ſeparated in the firſt minute, we have 101.5 for the true temperature of the mixture.

The original heat of the wheat was —	53.5	
That of the water was	126.75	
It follows, that the wheat was raiſed —	48	degrees
And the water cooled	25.25	

In this experiment the cold wheat was introduced into the veſſel containing the warm water. The heat received from the veſſel was equal to that which would have been received from .84 of an ounce of water. Hence by this experiment the comparative heat of water is to that of wheat as 2 to 1.

I have ſelected the foregoing experiments from a variety that were made with the ſame ſubſtances, becauſe they give the greateſt and leaſt reſults, when the mean of thoſe which were moſt accurate was taken, the capacity of water appeared to be to that of wheat as 2.1 to 1, which is probably near to the truth.

The reader will perceive that the ratio of the comparative heats of water and wheat, as deduced from theſe experiments, is conſiderably different from that which was publiſhed on the ſame ſubject in the firſt edition of this work. I was not then aware of ſome of the cauſes which tend to render the concluſions from ſuch experiments erroneous.

In the experiment on wheat formerly publiſhed, I endeavoured to aſcertain the temperature of the mixture by thermometers placed at the ſurface, and at the bottom; and as the mixture, in the courſe of the firſt minute, was not ſufficiently agitated to produce an uniform diffuſion of the heat, it became difficult to adjuſt the relative poſitions of the thermometers, in ſuch a manner as to determine the mean temperature with accuracy. The ſubſtances were alſo mixed in a wide-mouthed veſſel, which expoſed a large ſurface to the air, they therefore cooled rapidly; for theſe reaſons a conſiderable inaccuracy aroſe in the calculation of the comparative heats of wheat and water, as deduced from that experiment.

I muſt, however, obſerve, as an apology for this inaccuracy, that it did not ariſe from an error in the ſtatement of facts. I am convinced from the reſult of ſubſequent trials, that a ſeries of facts nearly ſimilar, would occur to others who ſhould take the trouble to repeat the experiments in ſimilar circumſtances.

I do not pretend to affirm, that in repeating ſuch experiments, a perfect coincidence of reſults can be obtained. In the work which I formerly publiſhed, I remarked, that a change in the temperature of the air in the room; a variation in the time that was employed in mixing together the ſubſtances, which are to have their comparative heats determined; a difference in the ſhape of the veſſel, or in the degree of agitation that is given to the mixture, will often produce a conſiderable diverſity in the reſult of the ſame experiment. But I can ſafely affirm, that nearly ſimilar reſults will be obtained when the experiments are repeated with a proper attention to theſe ſources of error; and the miſtake in the calculation of the comparative heat of wheat, as determined by the above-mentioned experiment, principally depended upon a fallacy in the deduction of the common temperature from facts, which were obſerved with attention, and ſtated with fidelity to the public.

It was before remarked, that in the 2d experiment, if the water and wheat had not been brought to a common temperature in the courſe of the firſt minute, leſs heat would have been apparently loſt in the ſecond repetition of the agitation than in the third. This is proved by the experiment which follows.

EXPERIMENT III.

Air in the room — 56
Twenty-two ounces of dried wheat at — 55
were mixed with 20 ounces of water at — 126.75

The circumſtances being the ſame as in the preceding experiments, excepting that the ſubſtances were not agitated when they were mixed together, the temperature in

1 minute was — 100.25
2 — — 100.75
3 — — 101 plus
4 — — 101.5
5 — — 102 minus

The mixture being then agitated 12 times, the temperature at the end of the

6th minute was — 103 plus
7 — — 103 plus
8 — — 103
9 — — 103
10 — — 103
11 — — 102.75

12	—	—	102.75	minus
13	—	—	102.5	
14	—	—	102.5	minus
15	—	—	102.25	
16	—	—	102.25	minus
17	—	—	102	
18	—	—	102	
19	—	—	102	minus
20	—	—	101.75	
22	—	—	101.5	
24	—	—	101	
26	—	—	100.5	
28	—	—	100	
30	—	—	99.5	
32	—	—	99	
34	—	—	98.5	
36	—	—	98	
38	—	—	97.25½	
40	—	—	96.75	
42	—	—	96	plus
44	—	—	95.5	

Thus it appears, that when the warmer and colder ſubſtances were not agitated at the beginning of the experiment, the thermometer continued to aſcend during the firſt five minutes, after which the mixture being agitated it ſuddenly roſe one degree. When I firſt obſerved this appearance on mixing water and wheat together, I ſuppoſed that it proceeded from the production of ſenſible heat, in conſequence of a chemical union between theſe ſubſtances. But I was ſoon led to correct that opinion, becauſe I found that the ſame effect was produced when water and ſand were mixed in ſimilar circumſtances. By varying the experiment, and by examining the temperature of the mixture at the ſurface and at the bottom, I diſcovered that the aſcenſion of the thermometer depended upon the unequal diſtribution of the heat, and upon the difference between the capacities of wheat and water.

For as no agitation was given to the mixture at the beginning of the experiment, the water and wheat were not brought to a common temperature, in the courſe of the firſt minute; and as the thermometer reached from the ſurface to the bottom, it indicated a degree which was intermediate between the temperature of the wheat and water. It was before obſerved, that this intermediate heat muſt be leſs than the common temperature *, becauſe the capacity of the warmer being greater than that of the colder ſubſtance, when they were brought to a common heat, the increaſe in the temperature of the latter would be greater than the diminution in that of the former. The thermometer, therefore, continued to aſcend until the mixture was brought by agitation in the ſixth minute to a common heat, after which it began ſlowly to deſcend. If the temperature of the wheat were greater than that of the water, the reverſe of this would take place; for in that caſe, the mixture being agitated as above, the thermometer would ſuddenly ſink ſeveral degrees.

By experiments ſimilar to thoſe which have been now recited, I endeavoured to determine the comparative heats of moſt of the farinaceous vegetables, and of ſome of the animal ſubſtances that are commonly uſed in aliment. The reſults are comprized in the following table.

Table of the comparative heats of vegetable and animal ſubſtances, water being the ſtandard.

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* See the obſervations on the experiments with ſand and water, page 112.

Common water	1.000
Horſe beans	0.502
Rice	0.506
Wheat	0.477
Oats having the hulls taken off	0.416
Peas	0.492
Unhulled barley	0.421
Lean of the beef of an ox	0.74
Hide of an ox with the hair	0.787
Lungs of a ſheep	0.769
Freſh milk of a cow	0.999
Arterial blood of a dog	1.03

In the courſe of my experiments with farinaceous vegetables, I found that when ſome of thoſe ſubſtances were reduced to powder, and intimately mixed with warm water, they produced ſenſible heat. This will appear from the experiment which follows.

EXPERIMENT IV.

Air in the room — 62

Twenty-two ounces of wheat coarſely powdered, being mixed with 18 ounces of warm water, and the mixture being reduced by agitation to a common heat, the temperature as meaſured by a thermometer which reached from the ſurface to the bottom

Was in 2 minutes		—	94½
3	—	—	94½
4	—	—	94½ plus
5	—	—	95 minus
6	—	—	95 plus
7	—	—	95¼
8	—	—	95½
9	—	—	95½ plus
10	—	—	95¾
11	—	—	95¾ plus
12	—	—	96 minus
13	—	—	96 minus
14	—	—	96 minus
15	—	—	96
16	—	—	96 plus
21	—	—	96 plus
26	—	—	96
31	—	—	95¾

The wheat ſwelled in the courſe of this experiment, and towards the concluſion was ſo much enlarged, as not to be completely covered by the water.

From the gradual aſcenſion of the thermometer during the ſpace of twenty minutes, it is manifeſt, that ſenſible heat muſt have been produced.

Agreeably to this I have found, that if wheat and warm water be mixed together, and ſuffered to cool, and if the mixture be heated a ſecond time, and again ſuffered to cool, the progreſs of the refrigeration, in the latter inſtance, will be much ſlower than in the former; which proves, that the water having remained for a conſiderable time in contact with the wheat, is at length enabled, by the aſſiſtance of a moderate degree of warmth, to contract a chemical union with it, and to produce ſenſible heat.

Senſible heat is likewiſe produced by mixing the powder of beans, peas, rice, oats, and barley, with water raiſed to 120.—My original intention in reducing theſe ſubſtances to a coarſe powder, was to bring them into ſuch a ſtate, as that they ſhould ſpeedily and intimately mix with water, this circumſtance being of importance to the accurate determination of their comparative heats.

They were pulverized by the following proceſs. Each of them was firſt ground into flour; it was then made into a thin cake with water, and baked in an oven till it became criſp; after which it was coarſely powdered by beating it in a mortar.

The coarſe powder of the farinaceous vegetables which is thus procured, will readily mix with water; whereas a conſiderable time, and much agitation, is required to mix the flour of theſe ſubſtances with that fluid.

When the powders obtained by this proceſs, were intimately mixed with water, and raiſed to a moderate degree of warmth, they were found, as was before remarked, to produce ſenſible heat; which rendered the reſults of the experiments made with a view to determine their comparative heats in ſome meaſure fallacious. I, therefore, had recourſe to the method which I had formerly adopted, of mixing thoſe ſubſtances with water in their entire ſtate; becauſe under theſe circumſtances, they either do not produce ſenſible heat, or the quantity produced is ſo ſmall, as not materially to affect the reſult of the experiment; and the comparative heats may, by this mode of trial, be determined with a conſiderable degree of accuracy, if the precautions formerly mentioned be properly attended to, and particularly if the warmer and colder ſubſtances, when they are mixed together, be placed in ſuch a ſituation, as that they ſhall cool very ſlowly. For, in that caſe, the heat loſt by the agitation neceſſary to bring them to a common temperature, will be inconſiderable.

The foregoing experiments prove in general, that fleſh, milk, and vegetables, contain leſs abſolute heat than water, and water leſs than arterial blood. Arterial blood, therefore, contains a greater quantity of abſolute heat, than the principles of which it is compoſed.

The remarkable accumulation of heat in this fluid, led me to ſuſpect, that it abſorbs heat from the air, in the proceſs of reſpiration. And in this ſuſpicion, I was much confirmed by the following conſiderations:

1. Thoſe animals which are furniſhed with lungs, and which continually inſpire the freſh air in great quantities, have the power of keeping themſelves at a temperature conſiderably higher than the ſurrounding atmoſphere. But animals that are not furniſhed with reſpiratory organs, are very nearly of the ſame temperature with the medium in which they live.

2. Among the hot animals, thoſe are the warmeſt, which have the largeſt reſpiratory organs, and which conſequently breathe the greateſt quantity of air in proportion to their bulk. Thus, the reſpiratory organs of birds, compared with their ſize, are more extenſive than thoſe of any other animal; and birds have the greateſt degree of animal heat.

3. In the ſame animal, the degree of heat is in ſome meaſure proportionable to the quantity of air inſpired in a given time. Thus we find that animal heat is increaſed by exerciſe, and by whatever accelerates reſpiration.

From theſe conſiderations, I was naturally led to a more particular examination of this ſubject: the reſult of which, is comprehended in the following propoſitions:

PROPOSITION I.

The quantity of abſolute heat contained in pure air, is diminiſhed by the change which it undergoes in the lungs of animals, and the quantity of heat in any kind of air that is fit for reſpiration, is nearly proportional to its power in ſupporting animal life.

Before I proceed to the direct proof of this propoſition, it is neceſſary to conſider the nature of the change produced in the air by the proceſs of reſpiration. It is now generally agreed among Philoſophers, that the air of the atmoſphere principally conſiſts of two different ſpecies of permanently elaſtic fluids, which have been called by Dr. Prieſtley phlogiſticated and dephlogiſticated air.

If a portion of atmoſpherical air be expoſed in an inverted jar to water, which has had its air ſeparated by boiling, it will be reſolved into the two fluids mentioned above; the purer part of it will be attracted by the water, and the noxious phlogiſticated part will remain in the veſſel unabſorbed.

Moreover, if phlogiſticated and dephlogiſticated air be mixed together in certain proportions, they will compoſe an elaſtic fluid which has a very near, if not a perfect reſemblance to common air. Theſe therefore appear, both by the ſynthetic and analytic mode of trial, to form the conſtituent parts of the atmoſphere, the dephlogiſticated air compoſing nearly one fourth of the whole, and the remaining three fourths conſiſting chiefly of phlogiſticated air.

Of theſe conſtituent principles, it is the purer part alone which is affected by the reſpiration of animals. This I think is evident from the following experiments.

If an animal be placed in a given quantity of atmoſpherical air, confined in a jar inverted over water, after a certain length of time, it will fall into convulſions and expire; the air will be diminiſhed nearly one fifth of its bulk, and that portion of it which remains in the jar will be found to conſiſt of phlogiſticated, mixed with a very ſmall quantity of pure air, the latter having been rendered by its diffuſion through the former, unfit for miniſtring any longer to the reſpiration of the animal. If the experiment be made over lime water, the liquor will become turbid, a precipitation will take place, and the quantity of calcareous earth precipitated, will be in proportion to the diminution of the air. If the trial be made over mercury, no ſenſible diminution will take place; but if a ſmall quantity of cauſtic alkali be introduced, it will be changed into the mild alkali, and the air will be diminiſhed in the ſame degree, as if the experiment had been made over water.

If the jar inverted over mercury, contain dephlogiſticated inſtead of atmoſpherical air, and if the animal placed in it be removed as ſoon as it begins to ſicken, and the cauſtic alkali be introduced, a much more conſiderable diminution will take place, and by the repeated introduction of the animal and of the alkali, almoſt the whole of the dephlogiſticated air may be made to diſappear *. Hence it follows, that it is the dephlogiſticated part of the atmoſphere, which miniſters to the ſupport of animal life, and which is altered by the action of the lungs.

It is extremely probable, that this alteration conſiſts in the converſion of pure into fixed air, by the union which the former contracts with inflammable air, or with its baſis, in the proceſs of reſpiration. That ſuch an union takes place in the lungs, will, I think, appear from the following conſiderations.

It is well known, that the blood undergoes a remarkable change of colour when circulating in a living animal, for the vivid

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* Vide Memoire ſur La Chaleur, par Monſ. Lavoiſier & De la Place, lû a l'Académie Royale des Sciences, le juin 28. 1783. p. 52.

arterial blood, in its paſſage through the capillaries to the venous ſyſtem, acquires a deep and livid hue, and again reſumes its light and florid colour in the lungs. Dr. Prieſtley has proved, that ſimilar alterations are produced in the colour of the blood by expoſure to pure and inflammable air. For when vivid arterial blood is expoſed to inflammable air, or to any other ſpecies of aerial fluid, known to contain that principle or its baſis, it ſpeedily acquires the deep and livid hue of venous blood. And, on the contrary, when venous blood is expoſed to pure air, it acquires the light and florid colour of arterial; and theſe alterations are produced in an equal degree, when a thin bladder is interpoſed between the air and blood. The ſame philoſopher has proved, that the air and blood which are employed in theſe experiments, undergo oppoſite changes, for pure air is vitiated by expoſure to venous blood; and on the contrary, inflammable air is abſorbed, and phlogiſticated air improved by expoſure to arterial blood: from the latter circumſtances it appears, that arterial blood has an attraction to inflammable air, or to its baſis.

That inflammable air will produce a change of colour in the blood, when introduced into the veins of a living animal, appears from the following experiment, which was tried by my ingenious friend Dr. Hamilton.—Three ligatures were made on the jugular vein of a cat, and the portion of the vein contained between two of them, having been previouſly emptied of its blood, was filled with inflammable air. The air being confined in this ſituation, by cloſing the puncture through which it was introduced, the middle ligature was removed, in conſequence of which, the blood that was contained in the part of the vein intercepted between the middle and the third ligatures, was ſuffered to mix with the inflammable air. After an hour had elapſed, the blood being drawn from the vein, was found fluid, and had acquired a colour almoſt as dark as ink. At the time when this trial was made, a quantity of blood, as nearly equal as poſſible to the former, was likewiſe intercepted, during the ſpace of an hour, between two ligatures that were tied on the crural vein of the ſame animal. This blood being drawn from the vein, was found in ſome meaſure coagulated,

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but not to ſo great a degree, as to be immiſcible with water. To theſe portions of blood, equal quantities of water were added; when it appeared, that the portion which was expoſed to the inflammable air, imparted a much deeper tincture to the water, than that which had been drawn from the crural vein. Hence it appears, that when inflammable air is introduced into the veins of a living animal, it increaſes the livid colour of the blood, and at the ſame time diminiſhes its tendency to coagulation. The inflammable air uſed in this experiment, was obtained from iron filings, diſſolved in the vitriolic acid. It cannot be doubted, that the ſame effect would be produced, by expoſing the blood to that ſpecies of inflammable air, which is obtained from animal ſubſtances.

Since, therefore, the arterial blood undergoes the ſame change of colour in the capillaries, that it ſuffers by expoſure to inflammable air; ſince it has an attraction to that fluid; and ſince the ſeparation of inflammable air from animal ſubſtances is promoted by heat, and by the tendency of the juices to putrefaction; we may I think ſafely conclude, that the change which the blood undergoes in the capillaries, ariſes from its impregnation with this principle. And as the abſorption of inflammable air, or of its baſis, is the cauſe of the change, which is produced in the colour of the blood, during its circulation through the capillaries; we may alſo conclude, that when the blood again recovers its florid colour in the lungs, the inflammable principle is detached. In this concluſion we may reſt with the greater certainty, becauſe the venous blood, in the lungs, is expoſed to the action of that ſpecies of air, which in other phlogiſtic proceſſes, is known to combine with the inflammable principle. In the proceſs of reſpiration, therefore, the pure air, which is received into the lungs, combines with a portion of the inflammable air contained in the venous blood.

The ſubſtances that are the reſult of the union between theſe fluids, in phlogiſtic proceſſes, differ from each other, becauſe inflammable air, which is one of the ingredients, is of different kinds. Thus the inflammable air obtained by the ſolution of metals, in the vitriolic acid has, in ſeveral reſpects, diſtinct properties, from that which is obtained, by expoſing animal and vegetable ſubſtances to heat in cloſe veſſels. The former has a much leſs ſpecific gravity than the latter; the firſt, when a lighted candle is dipped into a jar filled with it, explodes; the ſecond burns with a lambent flame; the reſiduum which ariſes from the combuſtion of the one, with a certain proportion of pure air, in cloſe veſſels, is water; but that which ariſes from the combuſtion of the other, in ſimilar circumſtances, is fixed air. It is evident, that the fixed air found in the latter caſe, muſt either have pre-exiſted in the inflammable air, or it muſt have been produced by the union of this fluid, or of ſome part of it, with dephlogiſticated air. From Dr. Prieſtley's experiments it appears, that it does not univerſally pre-exiſt in the inflammable air, for in ſome experiments, in which pure and inflammable air were mixed together, and fired by the electric ſpark, the quantity of fixed air, which was the reſult of the inflammation, was conſiderably greater than the entire quantity of inflammable air uſed in the proceſs *. Hence it is manifeſt, that either the whole or

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* See Prieſtley's Experiments relating to various Branches of Natural Philoſophy, vol. III. p. 272.

or a part of the fixed air found in thoſe experiments, muſt have been produced by a union of the inflammable air, or of ſome principle contained in it, with the dephlogiſticated air.

It appears therefore, that fixed air is produced by the combination of dephlogiſticated with lambent inflammable air, or with ſome of its conſtituent parts. It was before ſhown, that the laſt mentioned ſpecies of air, is united to the blood in the capillaries, and is again ſeparated from it, in the proceſs of reſpiration. And ſince it is found, that fixed air is exhaled by expiration, a portion of the pure air being at the ſame time made to diſappear, there is I think the utmoſt reaſon to believe, that the fixed air, which is the reſult of this proceſs, is produced by the union of the pure and inflammable air, which come into contact with each other in the lungs. To this concluſion it ſhould not be objected, that theſe fluids, when mixed together, do not ſuddenly combine, unleſs an ignited body be applied to them; becauſe we know from the experiments of Dr. Prieſtley, that inflammable air, without the aſſiſtance of flame, is capable, in its naſcent ſtate, of contracting an union with pure air.

As it has been proved, however, by the very ingenious experiments of Mr. Caven-diſh, that water is the reſult of the union between pure air, and the inflammable air which is obtained from metals, by the vitriolic acid; it is poſſible, that a portion of the pure and inflammable air, which meet in the lungs, may undergo that peculiar mode of combination by which water is produced. And this opinion will be rendered more probable, if we conſider that though the fixed air, which was the product of the abovementioned experiments that were made by Dr. Prieſtley, conſiderably exceeded the inflammable air, yet it was not equal to the ſum of the two airs, which were employed in thoſe experiments. From theſe conſiderations, I think we may conclude, that the pure air received into the lungs, combines with the inflammable principle, which is extricated from the blood, and that by this combination it is partly converted into fixed air, and partly into aqueous vapour *.

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* After I had written the above, I found that Dr. Higgins maintains the ſame opinion See experiments on acetous air.

In order therefore to determine the truth of the propoſition, it muſt be proved, that pure air contains a greater quantity of abſolute heat than the fixed air and aqueous vapour which are exhaled from the lungs †.

I have examined with great care the comparative quantities of heat contained in theſe fluids, and ſhall now proceed to communicate the reſult of my enquiry.

In the former edition of this work I obſerved, that from the imperfection of thermometers, and the difficulty of judging by the eye of the fraction of a degree, perfect accuracy in experiments for determining the heat of air, was not to be expected; and added, that I propoſed in future to endeavour to aſcertain with as much exactneſs as poſſible, the comparative quantities of heat in the different kinds of air, by a greater variety of trials, and by thermometers conſtructed for the purpoſe. I have ſince repeated my former experiments with as much accuracy as poſſible, and by giving a more minute attention to the circumſtances which influenced their reſults, I have endeavoured to make a nearer approximation to the truth. In conſequence of this, the reader will find, that the quantities of heat in the various kinds of air, as determined by my late experiments, are very different from thoſe recited in my former publication. It will appear, however, that the general facts on which the theory is founded are true, and I am perſuaded, that, with the candid and judicious, the novelty of the ſubject, and the extreme difficulty of the undertaking, will in ſome meaſure plead my apology for the errors in that work.

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† Some philoſophers have maintained, that the dephlogiſticated air is abſorbed by the blood. But this opinion does not appear to be ſupported by any deciſive experiments. Admitting it however to be true, it will not affect the general doctrine which I have endeavoured to eſtabliſh.—Becauſe if dephlogiſticated air be abſorbed, it muſt be ſeparated again by ſome of the emunctories: for if this were not the caſe, there would be an accumulation of it in the animal body. But it is not found that any of the ſecretions contain dephlogiſticated air. This fluid, therefore, ſuppoſing it to be received into the blood, muſt neceſſarily be altered in its properties; and it ſeems to be evident, that by coming into contact with the putreſcent parts of the ſyſtem, it would be changed into fixed air, or into water, and would conſequently have the quantity of its abſolute heat diminiſhed.

Before we can arrive at much preciſion in experiments, we muſt be acquainted with the various cauſes which may render their reſults fallacious; and in a new and obſcure branch of ſcience, a knowledge of theſe cauſes can only be obtained by repeated trials and attentive obſervation.

The peculiar difficulty of determining with preciſion the comparative heats of the different kinds of air, ariſes partly from the fugitive nature of heat, partly from the rarity of the permanently elaſtic fluids, in conſequence of which not more than fifteen or twenty grains of them can conveniently be employed in an experiment, and partly from the changes that they are liable to undergo, with reſpect to purity and the quantities of moiſture which they contain.

When the experiment is made by introducing a few grains of warm air confined in a bladder into an equal bulk of water, as the ſpecific gravity of the latter ſo greatly exceeds that of the former, it is manifeſt that very little heat will be imparted to the water. For the ſame reaſon, if different elaſtic fluids be employed, the differences in the quantities of heat communicated will be minute; and in that caſe, as from the imperfections of our ſenſes, and of thermometers, every experiment is neceſſarily attended with ſome degree of inaccuracy, the errors of the obſervations will bear a conſiderable proportion to the differences which are to be aſcertained; the reſults will therefore be liable to much uncertainty.

A variety of expedients were tried with a view to obviate this difficulty; a few of which I ſhall briefly relate, as they enabled me to aſcertain ſome of the moſt important circumſtances that require attention, in trials for determining the heat of air, and led to a mode of making the experiment which appears to be deciſive. My firſt expedient was as follows:

In the ſide of a crooked glaſs tube, that was of ſuch a ſize as to contain nearly half an ounce of iron filings, a ſmall aperture was made, through which a very ſenſible thermometer was introduced into the centre of the tube, being kept in its place by means of a cork cloſely adapted to the aperture. A bladder having a braſs cock tied to its neck, was occaſionally fixed to the lower end of the tube. The air, which was to be the ſubject of an experiment, being confined in this bladder, was raiſed to the required temperature in a Dutch oven, and was then forced through the filings contained in the tube, by a preſs which had a weight applied to it. Thus a conſiderable quantity of warm air was made to paſs through a ſmall quantity of cold iron filings. It is proper to obſerve, that the bladder was ſurrounded by ſeveral folds of flannel, in order that the heat might be retained in the air, during the ſhort time which was employed in forcing it through the tube. As complete a ſeparation as poſſible was made, between the bladder and tube, by means of thick flannel.

With this apparatus I made the following experiments, which, as they tend to throw light upon the ſubject of the preſent enquiry, ſeem not unworthy of attention, though they do not lead to any direct concluſion reſpecting the object which I had principally in view.

EXPERIMENT I.

Fourteen ſcruples of dry iron filings being introduced into the crooked glaſs tube, and the thermometer in the centre ſtanding at 49½; nearly 40 ounce meaſures of atmoſpherical air were heated in the bladder to 110, and made to paſs through the filings by means of the preſs and weight mentioned above. In one minute and ſeven ſeconds nearly, the thermometer ſtood at 57. It was therefore raiſed 7½ degrees. I continued to repeat this experiment for the greater part of the day, with the ſame bladder, and found, that the heat communicated to the filings gradually diminiſhed, till at length the reſult was as follows. The thermometer in the centre being 48½ nearly, 40 ounce meaſures of atmoſpherical air were raiſed to 118, and paſſed through the filings as above. In one minute, the thermometer ſtood at 50½. It was therefore now raiſed only 2 degrees. Though I repeated the laſt experiment with all the attention I was capable of, yet I found the filings in ſuch a ſtate, that not more than 2 degrees were communicated by the hot air which was forced through them. I then removed the filings from the inſtrument, and perceived by their adhering to each other, and to the inner ſurface of the tube, that they had collected a conſiderable quantity of moiſture. The filings and tube being made perfectly dry, and the experiment again repeated ed on the enſuing day, the thermometer was raiſed as at firſt more than 7 degrees.

The air being made to paſs repeatedly through the ſame filings, the quantity of heat which it communicated, gradually diminiſhed, and the filings were found to have contracted moiſture, as in the former experiments.

That the moiſture imparted to the filings in theſe inſtances was derived from the bladder, is proved by the experiments which were made with the following apparatus. See plate 1. fig. 6.

A copper worm H K was fixed in a wooden veſſel F G, which was occaſionally filled with warm water, or with melted ſpermaceti; one extremity of the worm M, terminated in a pipe which paſſed through the ſide of the veſſel, and which was ſo contrived, as to be capable of ſlipping into a braſs tube connected with the bottom of the ſmall glaſs veſſel A B, that contained iron filings, the pipe and tube being made to fit each other with ſuch exactneſs, as to be air tight. By a ſimilar contrivance, the other extremity of the worm was joined to the bladder C D, in which the air was contained. At the point E, a thermometer was inſerted in the worm, to ſuch a depth, that the current of air muſt neceſſarily ſtrike againſt its bulb. To the upper extremity of the glaſs veſſel A B containing the filings, was fixed a crooked tube A L, the farther end of which was introduced into the mouth of an inverted glaſs jar R S, that was placed in the wooden veſſel P Q. To this jar a ſcale was applied, by the help of which the quantity of air employed in the experiment was accurately meaſured.

It is manifeſt, that the air expelled from the bladder by means of the preſs formerly mentioned, would in paſſing through the worm acquire the heat of the ſurrounding fluid, and having imparted its heat to the cold filings contained in the glaſs tube A B, would afterwards be received into the inverted jar R S, that had been previouſly filled with water.

I was at firſt induced to contrive this apparatus, hoping that I ſhould have been enabled to determine by it, the comparative heats of the different kinds of air. In that expectation, however, I was diſappointed. But, as was before obſerved, the trials that were made with it, clearly proved the effect of the moiſture derived from the bladder in influencing the reſults of ſuch experiments. It may not be improper briefly to relate, the experiments by which this was aſcertained.

EXPERIMENT II.

A quantity of melted ſpermaceti at 107, having been poured into the veſſel F G, nearly 28 ounce meaſures of atmoſpherical air included in the bladder, were made to paſs by means of the preſs and weight through the worm and filings.

In this experiment, the air in the bladder had the ſame heat with that in the room. During its paſſage through the worm, it acquired the heat of the melted ſpermaceti; as appeared from the thermometer employed to indicate its temperature. This heat it imparted to the cold iron filings, which were raiſed by it, in one minute, four degrees.

EXPERIMENT III.

The melted ſpermaceti being again introduced into the veſſel, 28 ounce meaſures of atmoſpherical air, included in the ſame bladder, which had not been uſed for a conſiderable time, were gradually raiſed in a Dutch oven to the temperature of 95. The air was then forced through the worm and filings, and the heat imparted to the latter, in one minute, was 8 degrees.

In both theſe experiments, the air had acquired the temperature of the melted ſpermaceti, as was evident from the thermometer inſerted in the worm; and from the difference of the heat imparted to the filings, it is manifeſt that, in the ſecond experiment, the warm bladder muſt have communicated moiſture to the air contained in it; but in the firſt, as it had not been previouſly heated, it was not in a condition to produce that effect. Hence we may perceive the cauſe of the gradual diminution of the heat, in the experiments recited in page 159.

The bladder uſed in theſe experiments, had imbibed moiſture in conſequence of its expoſure to the external air. When it was firſt heated, it communicated a portion of this moiſture to the air contained in it, and the air in theſe circumſtances, being filtered through the cold dry filings, the vapour which was mixed with it was condenſed, the filings became moiſt, and much ſenſible heat was produced: but in the ſucceeding experiments, as the bladder was more perfectly dried, in conſequence of being more frequently heated, it communicated a ſmaller quantity of moiſture to the air which it contained; and conſequently the latter, in paſſing through the damp iron filings, gradually parted with leſs and leſs aqueous vapour, till at length it would not any longer give off moiſture, but would rather be in a condition to abſorb it.

Agreeably to this I have found, that when the experiment was made by heating air confined in a bladder, and immerſing it in cold water, the heat imparted to the water was greater or leſs, in proportion as the bladder communicated to the air which it contained, a greater or a leſs quantity of moiſture.

Of this I ſhall mention the following inſtances.

Fourteen ounce meaſures of atmoſpherical air were introduced into a ſmall bladder, which weighed very nearly a drachm and an half, and which had been perfectly dried, having been previouſly freed from its oil, by waſhing it carefully in ſoap and warm water.

The temperature of the room being — — 50,

The air contained in the bladder was gradually raiſed in a Dutch oven to	101,
And was ſuddenly immerſed in 18 ounces of water, which had the temperature of — —	48.6,
In three minutes the water was — —	49.1 nearly.

The air, therefore, with the bladder in which it was confined, communicated to the water .5 of a degree nearly.

But if great care was not taken to free the bladder from moiſture, the heat imparted to the water in ſimilar trials, was not leſs than 8 or 9 tenths of a degree. And when bladders ſomewhat larger were uſed, as in the experiments which were formerly publiſhed on this ſubject, the quantity of heat communicated was more than a degree.

It appears, therefore, that theſe experiments are liable to great variations, from the moiſture imparted by the bladder to the air which it contains. When the trial was made with the apparatus deſcribed above, the reſult was alſo affected by the moiſture which the iron filings might contain; and as the air neceſſarily flowed through a portion of a cold pipe, in its paſſage to the crooked tube, much heat was diſſipated before it came into contact with the filings. It is manifeſt, that the calculation of the heat which was thus loſt, muſt be attended with much uncertainty.

Finding that theſe methods of making the experiment, were liable to ſo many ſources of fallacy, I next endeavoured to aſcertain the relative capacities of the various kinds of air, by heating them in Florence flaſks, and immerſing them in water; the temperature of the air being meaſured by a very ſenſible mercurial thermometer, introduced into the centre of the flaſk through a cork adapted to its mouth, and that of the water, by ſimilar thermometers, which had each degree of Farhenheit's ſcale divided into ten equal parts.

In my firſt attempts to determine the comparative heats of the permanently elaſtic fluids by this apparatus, the Florence flaſk was filled with water previouſly to the introduction of the air. But I ſoon found, that the quantity of moiſture which adhered to the internal ſurface of the veſſel, rendered the reſult of the experiment precarious. For this reaſon, in my ſubſequent trials, the flaſk, before the air was introduced into it, was filled with mercury.

From theſe trials it clearly appeared, that dephlogiſticated air communicated more heat to water, than fixed or phlogiſticated air, in ſimilar circumſtances.

It was found, however, from the errors produced by variations in the relative poſitions of the flaſk, and of the thermometers, and from the difficulty of introducing the former into the water, with equal velocities, in different experiments, that the comparative heats of the permanently elaſtic fluids, could not be determined by this mode of trial with ſufficient accuracy.

It may be proper to add, that in ſome of the experiments which were inſtituted in the courſe of this enquiry, I employed a ſpirit of wine thermometer, that had each degree divided into an hundred equal parts diſtinctly viſible by the naked eye. The preference was given to that fluid for theſe experiments, as it is much more expanſible by heat than mercury. With this inſtrument, I hoped that I ſhould have been able to have diſtinguiſhed minute variations of heat; but it did not anſwer my expectations.

For when thermometers are conſtructed in ſuch a manner, that the ball or cylinder in which the fluid, that meaſures the heat is contained, ſhall bear ſo great a proportion to the ſize of the bore, as that each degree may be ſubdivided into many equal parts, what is gained in the magnitude of the ſcale, is loſt in the diminiſhed ſenſibility of the inſtrument. When the above-mentioned thermometer, for example, was immerſed in water that had a higher temperature than itſelf by $\frac{1}{8}$ of a degree, it did not acquire the heat of the water, in leſs than a minute and a half. And when the ſame thermometer, together with a very ſenſible mercurial one, were placed in a ſmall veſſel containing water to which heat was applied externally by the hand, the mercury roſe ſeveral degrees higher than the ſpirit of wine.

Since, therefore, large thermometers, which are made with ſpirits of wine, are very defective in point of ſenſibility; and ſince it appears, that when they are applied to a ſubſtance which has a variable heat, they never indicate its true temperature; they are totally improper for nice experiments, in which minute variations of heat are to be aſcertained *.

I likewiſe made trial of air thermometers, with a view to determine the comparative heats of the permanently elaſtic fluids; but theſe, though very ſenſible, did not anſwer the purpoſe, being deficient in point of accuracy.

In the courſe of my experiments on this ſubject, fixed and phlogiſticated air were found to undergo a remarkable change, when expoſed to heat in bladders.

This will appear from the experiments which follow.

Air compleatly noxious was obtained by forcing freſh air into the touch-hole of a gun barrel, the butt-end of which containing a ſmall quantity of roſin, was expoſed to a red heat.

The air thus produced was received, by means of a crooked tube fixed to the mouth of the barrel, in an inverted jar filled with water.

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* It will be ſhewn in the ſequel, that a method was diſcovered of employing, with advantage, large mercurial thermometers, to determine the comparative heats of the permanently elaſtic fluids.

water. Being carefully examined by Dr. Prieſtley's teſt, it was found, that two meaſures of it mixed with one of nitrous air, occupied the ſpace of three meaſures. A quart and half a pint of this air, was then introduced into a thin bladder, ſurrounded with a covering of flannel, and was gradually raiſed in a Dutch oven placed before a fire in the room, to the temperature of 130. After which, it was very nearly as good as common air, two meaſures of it, and one of nitrous air, occupying the ſpace of two meaſures *. It is neceſſary to obſerve, that this experiment was made in a very thin hog's bladder, which had been previouſly well rubbed.—If the experiment be made in a thick bladder, that has not been previouſly rubbed, it will not ſucceed.

A ſimilar improvement was made in fixed air by expoſure to heat in bladders. It is proper, however, to remark, that in the latter

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* When air newly obtained from roſin, in the manner above deſcribed, was expoſed to heat, a penetrating ſmell was perceived to iſſue from the bladder in which it was confined, but this effect was produced in a much ſlighter degree, when the air, previouſly to its expoſure to heat, had been kept for a conſiderable time in contact with water.

ter ſpecies of air, the change was effected with greater difficulty; more heat, and a longer time being required to produce it.

I ſhall obſerve in the laſt place, that if air confined in a bladder, and heated as above, be immerſed in water, which is nearly of the ſame temperature with the atmoſphere, leſs heat will be imparted, all other circumſtances being equal, than if it had been immerſed in water ſeveral degrees below the temperature of the atmoſphere; a portion of the heat communicated, in the latter inſtance, being evidently derived from the external air, in conſequence of the agitation given to the water, by immerſing in it a body that has a large ſurface. The error ariſing from this cauſe may be eſtimated by the following experiment.

Air in the room	75·5,
A pint of water in a tin veſſel being found, when tried by a very accurate thermometer, that had each degree divided into ten equal parts, to have the temperature of	74.5,
And a pint of atmoſpherical air, which was contained in a bladder, and which had preciſely the	

ſame heat with the air in the room, being ſuddenly immerſed in this water, the latter was raiſed to 74.6,

Agreeably to this I have obſerved, that if phlogiſticated air, which had been made 50 degrees hotter than the air in the room, was immerſed in water one degree below the temperature of the atmoſphere, it would communicate more heat, by nearly one-tenth of a degree, than if it had been immerſed in water, only half a degree below the temperature of the atmoſphere.

The water uſed in the above experiment, had been raiſed to 75.1, after which it had remained in the room more than an hour; during this time it was cooled by evaporation to 74.5, and at this point it remained ſtationary, the air in the room being 75.5.

In a former experiment, the air in the room being 72.5, and the ſame thermometer being introduced into a pint of water that was raiſed in the ſame veſſel to 72.5, the temperature of the water was found to fall, in a ſhort time, to 72, the air remaining at 72.5.

The laſt experiment was repeated with a ſimilar reſult, when the air was at 65.

This ſeems to prove, that the evaporation from the ſurface of water, is not always in proportion to the heat of the external air; but is ſometimes the ſame at different temperatures, and different when the temperatures are the ſame.

From theſe facts and obſervations it will appear, that the following circumſtances require attention, in experiments for determining the comparative heats of the permanently elaſtic fluids.

1. The portions of the aerial fluids that are to be ſubjected to the experiment, ſhould be free from moiſture: or at leaſt the quantities of moiſture which they contain, ſhould be as nearly equal as poſſible.

2. The trials ſhould be made in ſuch a manner, that thoſe fluids may undergo no change during the experiment.

3. If the experiment be made by immerſing air in water, the temperature of the latter, previouſly to the immerſion, ſhould be ſomewhat lower than that of the air in the room; and it is of great importance, that the difference between the heat of the ſur-

rounding atmoſphere, and of the water, before the air is introduced into it, ſhould be the ſame in different experiments.

4. The air ſhould be introduced into the water as ſpeedily as poſſible; and in different experiments, the velocities of introduction ſhould be equal.

5. In order to magnify the differences between the heats communicated by the various kinds of permanently elaſtic fluids, it is proper to raiſe them to a high temperature, previouſly to their immerſion in the water; for the heats imparted by thoſe fluids, and conſequently the differences of the heats which they communicate, will be nearly in proportion to the exceſs of their temperatures, above that of the water in which they are immerſed.

This, however, muſt be underſtood with ſome limitation; for when the temperature of the air that is to be employed in the experiment is very high, a part of the water will be converted into vapour at the moment of immerſion, and much heat will be loſt. If oil be uſed as a ſtandard inſtead of water, the raiſing of the air to a very high degree of heat, will moreover be ſubject to this inconvenience, that a conſiderable time will be required, after immerſion, to bring the air and oil to a common temperature. I have found in general, that the air may be raiſed with advantage, nearly to the heat of boiling water.

6. It is of importance to employ thermometers, which are very ſenſible, and which at the ſame time have as large a ſcale as poſſible. Theſe qualities are with difficulty combind in the ſame inſtrument. They may, however, be in ſome meaſure united by uſing ſmall mercurial thermometers with very fine bores.

Of the different fluids that have hitherto been tried for the purpoſe of meaſuring minute variations of heat, mercury is decidedly the beſt; for air is defective in point of accuracy, and all other fluids, excepting air, are inferior to mercury in ſenſibility.

7. In different experiments the thermometer ſhould be kept preciſely at the ſame diſtance from the bladder or veſſel containing the air, and from the ſurface and bottom of the water.

8. It is of great importance that the air, and the fluid to which it imparts its heat, ſhould ſpeedily acquire a common tempera-

ture; and, on the contrary, that the fluid to which the heat is imparted, ſhould give off that heat as ſlowly as poſſible to the ſurrounding atmoſphere.

With theſe circumſtances in view, I contrived an apparatus for determining the comparative heats of the different kinds of air, the outlines of which are expreſſed in plate II.

AB, CD, repreſent two ſlight braſs cylindrical veſſels, of equal weights and capacities, terminating in the ſmall tubes *l s*, *r k*, at the extremities of which are fixed the braſs cocks *s* and *k*. Theſe cocks are joined to the croſs bar MN, which conſiſts of two elaſtic braſs plates that are ſcrewed together at the middle, but are capable of being ſeparated from each other, to a little diſtance, at their extremities; in conſequence of which the necks of the cocks, that are fixed to the ſtems of the braſs veſſels, can be ſlipped in between them, where they are lodged in grooves formed for receiving them, and are made faſt by ſmall ſcrews that paſs through the extremities of the plates. To the middle of the croſs bar is fixed a ſquare ſocket, the inner ſurface of which is finely poliſhed, and which is of ſuch a ſize as to be accurately adapted to the poliſhed upright bar E F, and at the ſame time eaſily moveable upon it. At equal diſtances from the upright bar are placed the tinned veſſels G H, I K, that have a little more than double the capacity of the braſs cylinders. The diſtances of the veſſels and cylinders from the upright bar are ſo adjuſted, that when the cylinders are made to deſcend, they ſhall be directed by the bar and ſocket preciſely into the centres of the tinned veſſels, and when, in deſcending, they have arrived at $\frac{1}{8}$ of an inch from the bottoms of thoſe veſſels, they are ſtopped by means of a pin fixed in the upright bar at the point T. The cylinders being formed for the purpoſe of receiving the airs, which were to have their comparative heats determined, were made very ſlight, in order that the quantities of the airs, might bear as great a proportion as poſſible, to the weight of the veſſels in which they were contained. With a view to diminiſh the inaccuracy unavoidably ariſing from the heats communicated by the cocks *s*, *k* and by the croſs bar M N, the tubes *l s*, *r k*, were made very ſlight, and of a conſiderable length; the diſtance from the cocks to the braſs cylinders, being four inches and ſeven-eighths. From the length and ſlightneſs of the tubes, it was found that the braſs cylinders could not be kept ſufficiently ſteady, in the centres of the tinned veſſels, by the croſs bar M N. For this reaſon another bar O P was added, a lateral view of which is ſeen in figure 1ſt, plate III.

a b repreſents a narrow braſs plate of a conſiderable thickneſs, to which is fixed the ſocket *o p*, moveable upon the upright bar formerly deſcribed. The two thin plates *c n*, *d k*, are riveted to *a b*, at the points *o* and *p*, and their extremities *l* and *m* are formed into grooves which are adapted to one half of the circumference of the ſtems of the braſs cylinders, to the other half of which are cloſely applied two braſs pieces, which are alſo grooved, and which are faſtened to the plates by the ſcrews *r* and *s*. The two male ſcrews, *g* and *h*, are paſſed through the extremities of the thick plate *a b*. Theſe are introduced into two correſponding female ſcrews in the thin plates *c n*, *d k*, by means of which, it is evident that the latter may be made to approach nearer to the former, or to recede to a greater diſtance from it. This motion is communicated to the ſtems of the braſs cylinders, by means of which they are adjuſted; in ſuch a manner, as that, when they are introduced into the water, they ſhall be accurately in the centres of the tinned veſſels, where they are kept ſteadily in their proper poſitions. The ſuperior and inferior croſs bars are connected together by a thin plate of braſs, which paſſes from the ſocket of the former to that of the latter, and which is made faſt by means of ſcrews. The ſcrew that fixes this plate to the ſocket of the inferior bar, is of ſuch a length, as, when ſcrewed home, to extend to a ſmall diſtance beyond its inner ſurface, by which means the apparatus, in deſcending along the upright bar, may be faſtened to any part of it.

To each of the tinned veſſels two very ſmall mercurial thermometers are fixed, which are of different lengths, the longer reaching as far as the centre of the veſſel, the ſhorter extending a little lower than the ſurface.

The thermometers *a b* and *c d*, (ſee the figure, plate II.) which indicate the heat in the veſſel G H, are applied to a thin plate of ivory *e f*, which has two ſcales divided upon it, one for each thermometer. This plate is fixed to the veſſel by means of a clamp *g h*, the ſhank of which (*g h*) conſiſts of two thin elaſtic braſs plates, united at the top, and ſeparable from each other at the lower extremity. For the purpoſe of admitting the thermometers, the projecting part *o v*, having the figure of a paralelopiped, was added to the tinned veſſel. The exterior ſide of this projection, is received between the plates of the ſhank deſcribed above. Theſe plates are cloſely applied to it by means of their elaſticity, in conſequence of which the clamp is ſteadily retained in its poſition, at the ſame time that it admits of a conſiderable degree of elevation or depreſſion. Hence the depth to which the thermometers are ſunk, may be varied at pleaſure; and in order that this depth may be accurately known, a ſcale of equal parts is divided upon the exterior plate of the ſhank. Through the upper part of the ſhank is paſſed a ſmall male ſcrew S, ſimilar to that which is uſed in adjuſting the focus of a teleſcope. This is introduced into a correſponding female ſcrew inſerted in the ivory plate, which has the thermometers fixed to it; and as that plate is ſuſpended in the clamp by a pin, around which it has a free motion, it is manifeſt that by turning the above-mentioned ſcrew, the thermometers may be made to approach nearer to the centre of the veſſel, or to recede farther from it.

For the purpoſe of admitting a third thermometer, which was uſed in ſome experiments, a ſmall female ſcrew was ſoldered obliquely near to the bottom of each of the tinned veſſels. Through this the thermometer was introduced into the veſſel, and was made faſt by means of a male ſcrew cemented to its ſtem. Theſe ſcrews preſſed upon each other by flat ſurfaces, which were ground with ſuch accuracy, as to be oil or water tight, without the help of leathers. Two ſmall mercurial thermometers *l* and *r* (ſee fig. plate II.) were introduced by means of ſcrews into the centres of the braſs cylinders, for the purpoſe of meaſuring the heats of the airs which they contained. Theſe thermometers have the degrees divided upon their ſtems by a diamond.

The following method was employed to free the air from moiſture, and to introduce it into the braſs cylinders: ſee plate I. fig. 7. *a b c d* repreſents a pewter ſyringe, which is occaſionally filled with water, and inverted in a veſſel containing the ſame fluid. The ſyringe being placed in this ſituation, the air, which is to be the ſubject of the experiment, is introduced into it by means of a glaſs funnel. When the air is introduced, a ſmall pipe *g h*, which has the cock *g* fixed to it, is ſcrewed to the extremity of the ſyringe below the ſurface of the water. The cock being open, the piſton (*n o*) is forced a little downward, to expel the water with which the pipe *g h* is unavoidably filled, when it is ſcrewed to the ſyringe. Upon this the cock being cloſed, and the ſyringe being raiſed from the water, it is ſcrewed to the worm *k l*, that is fixed in the wooden veſſel *i m*, and is ſurrounded with a mixture of ſnow and ſalt.

A flaccid bladder previouſly freed from air, by rolling it with packthread, and having a braſs cock *p* firmly attached to its neck, is ſcrewed to the extremity of the worm *l*. The communication through the worm is then opened by turning the cocks *p* and *g*; and the air is gradually forced from the ſyringe into the bladder, in which it is confined by ſhutting the cock. The bladder containing the air is next ſeparated from the worm, and is ſcrewed to the extremity of the cock, which is fixed to the ſtem of the braſs cylinder, the latter having been previouſly exhauſted by an air pump; the communication is now opened by turning the cocks, and the air ruſhes from the bladder into the cylinder.

The worm is ſurrounded with a mixture of ſnow and ſalt, that the air, by the cold to which it is expoſed in its paſſage, may be as much as poſſible freed from moiſture. The experiments which have been made to prove that ice evaporates, cannot properly be objected to this method of ſeparating moiſture from the air. For my friend, Mr. Wilſon, has diſcovered by ſtatical experiments, that at very low temperatures, no evaporation takes place from the ſurface of ſnow *.

In trials for determining the comparative heat of air, by means of the apparatus now deſcribed, it is of great importance that the worm, the bladder, and braſs cylinder, ſhould be perfectly dry; and this circumſtance being properly attended to, I have not found any difference to take place, between the heat

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* It has been proved by Wallerius, that the evaporation of ice is greateſt at the moment of congelation; and that it diminiſhes when the cold is increaſed. See Wallerius Memoir de l'Academie de Suede, 1746. The experiments of Wallerius on this ſubject are confirmed by thoſe of Muſchenbroke.

communicated to water by air, which, previouſly to its introduction into the cylinder, had been cooled to a low temperature, and by that which had been introduced at the temperature of the air in the room. It is proper, however, that the airs ſhould be introduced at a temperature ſomewhat lower than that of the water in the tinned veſſels; becauſe we are then certain that they do not contain any aqueous vapour, capable of being condenſed by the fluid in which they are immerſed.

From Sauſſeur's experiments it appears, that the quantity of aqueous vapour attracted by the air of the atmoſphere, when at 65 of Farhenheit, is very ſmall, a cubic foot of air requiring not more than eleven or twelve grains of vapour to bring it from the ſtate of perfect dryneſs to that of extreme moiſture. Hence it follows, that not more than $\frac{1}{3}$ of a grain would be neceſſary to ſaturate with aqueous vapour, 37.9 cubic inches of air, which is equal to the ſolid content of one of the cylinders. It has alſo been proved by the ſame Philoſopher, that minute quantities of water are ſufficient to ſaturate the other permanently elaſtic fluids. When we find, therefore,

fore, that the aërial fluid, which is to be the ſubject of our experiment, is at any of the intermediate degrees, between the points of ſaturation and extreme dryneſs, we are ſure that it contains a very ſmall quantity of moiſture.

The braſs cylinders were heated by the following contrivances, ſee plate III. figure 2. A B repreſents a quadrangular tinned veſſel, that has the two ſmaller veſſels C D, E F, fixed to its bottom by means of dovetails, along which they may be made to ſlide, and may be ſo adjuſted that their diſtances from each other, ſhall be equal to the diſtances of the cylindrical veſſels in which the air is contained; their ſize being ſuch that they ſhall be nearly accurately filled by thoſe veſſels, when the latter are introduced into them. It is, therefore, manifeſt that warm water contained in the large veſſel A B, and coming into contact with the external ſurfaces of the ſmaller veſſels C D, E F, will neceſſarily communicate a part of its heat to thoſe veſſels, and to the cylinders which are placed in them. If the cylinders had been heated by being immediately immerſed in warm water, a quantity of moiſture would have adhered to their ſides, which would have produced an inaccuracy in the experiment.

In conſtructing the above deſcribed apparatus, which was made by Meſſrs. Nairne and Blunt, great care was taken that the braſs cylinders ſhould be preciſely equal in weight, dimenſions, and capacity. Their weight is 7 ounces, 3 drams, 38 grains troy each; the length is 8 inches and $\frac{7}{8}$; the breadth 2 inches $\frac{3}{8}$ near to their upper extremity, and 2 inches $\frac{5}{8}$ near to the bottom. They are each of them capable of containing twenty ounces Troy of water, and conſequently the ſolid content is 37.91 cubic inches. The tinned veſſels are alſo equal to each other in weight and dimenſions. They are covered with thick flannel for the purpoſe of confining, as much as poſſible, the heat imparted to the water by the cylinders.

The bulbs of the thermometers were not more than $\frac{2}{10}$ of an inch in diameter, in conſequence of which they were very ſenſible; and their bores were ſo minute as to admit of a large ſcale, each degree of Farhenheit being divided into ten equal parts, diſtinctly viſible to the naked eye. It was found a very difficult undertaking to graduate four thermometers of ſo ſmall a ſize, with ſuch preciſion, as accurately to correſpond with each other. After many attempts, in the courſe of ſeveral months, it was at length accompliſhed. They were ſo exactly adjuſted, as not to differ from each other more than the $\frac{1}{10}$ of a degree in any part of the ſcale, excepting in a ſmall portion of it near to the beginning, where the variation was $\frac{2}{10}$ of a degree. That the difference might be ſtill farther diminiſhed, a ſcale of the errors was formed, by immerſing the thermometers in a veſſel containing a large quantity of warm water, and by accurately examining the contractions in each thermometer, which correſponded to the diminutions of heat in the water.

I ſhall now proceed briefly to relate the experiments which were made with this apparatus. The following was tried with a view to examine the accuracy of the inſtrument.

EXPERIMENT I.

The cylinders having been previouſly made perfectly dry, and having been exhauſted by means of an air pump, equal quantities of atmoſpherical air were introduced into them. They were placed in the water bath formerly deſcribed, and were expoſed to the action of boiling water for ten minutes.

Into each of the tinned veſſels G H, I K, ſee fig. plate II. was introduced 20 ounces Troy of water, having very nearly the ſame temperature with the air in the room. The water bath and cylinders being now removed from the fire, were placed upon a ſtand below the table, on which the upright bar and tinned veſſels were fixed. The cylinders were then inſtantly raiſed from the bath, and were introduced, by means of the ſocket and upright bar, into the centre of the water in the tinned veſſels, where they were kept faſt by a ſcrew applied to the bar. Having been ſuffered to remain in this ſituation for five minutes, they were removed; the water in the veſſels during a few ſeconds was equally agitated; after which the temperatures being examined by the central thermometers, with the help of a magnifier, were found to correſpond within leſs than the $\frac{1}{20}$ of a degree. This experiment was ſeveral times repeated with a ſimilar reſult, the cylinders being ſuffered to remain immerſed in the water, during the ſpace of eight, twelve, and ſixteen minutes ſucceſſively. The cylinders were heated in a room adjoining to that in which the tinned veſſels containing the water were placed; becauſe a fire in the latter, by diſturbing the equality of its temperature, would have rendered the experiment liable to inaccuracy.

The tinned veſſel and braſs cylinder, placed towards the right hand during the examination of the thermometers, may be diſtinguiſhed by the letter R; thoſe towards the left by L.

EXPERIMENT II.

The cylinder L was filled in the manner deſcribed above with dephlogiſticated air, the purity of which, when it was examined, at the concluſion of the experiment, was ſuch that one meaſure of it, mixed with two of nitrous air, occupied the ſpace of one meaſure.

Freſh atmoſpherical air was introduced into the cylinder R.

Twenty ounces Troy of water was contained in each of the tinned veſſels.

The air in the room was 58.1.

The water in the veſſels being agitated for a few ſeconds, the temperature in R was found to be 57.8; that in L, 57.8.

The cylinders having been previouſly exposed, in the water bath, to the action of boiling water, for 10 minutes, were ſuddenly introduced, as in the former experiments, into the tinned veſſels. They were kept in this poſition for 14 minutes, during which time the temperatures, examined by the central thermometers, were as follows:

	R		L	
1 min.	58.8	a little more	59.1	
2 -	59.1	—	59.3	
3 -	59.2	—	59.4	a little leſs
4 -	59.2	—	59.4	
5 -	59.3	—	59.5	a little leſs
6 -	59.3	—	59.5	very nearly
7 -	59.3	—	59.5	
10 -	59.4	—	59.6	a little leſs
14 -	59.5	—	59.7	a little leſs

The cylinders being then removed, and the water agitated for a ſhort time, the general temperature in the veſſel R was a little leſs than 60; that in L 60.2.

The air in the room was 60.25, very nearly.

This experiment was repeated with the following reſult.

Ex-

EXPERIMENT III.

Air in the room — 58.6
Water in each of the
tinned veſſels - 59.5 very nearly.

Dephlogiſticated air, having the ſame degree of purity with that uſed in the preceding experiment, was introduced into the cylinder L, and atmoſpherical air into R.

The cylinders were then heated, and immerſed in the tinned veſſels, in the manner deſcribed above; and the temperatures being accurately examined by the central and ſuperficial thermometers, were as follows.

	R		L	
Min.	Centre.	Surface.	Centre.	Surface.
2	61.1 *p.*	62.9	61.2 *p.*	63.1
3	61.1 *p.*	62.9	61.3 *m.*	63.1
4	61.2 *m.*	62.8 *p.*	61.4 *m.*	63.1
5	61.2 *p.*	62.8 *p.*	61.4 *m.*	63 *p.*
6	61.2 *p.*	62.8 *p.*	61.4	63 *p.*
7	61.2 *p.*	62.8	61.4	63 *p.*
8	61.2 *p.*	62.8	61.4	63 *p.*
9	61.2 *p.*	62.7 *p.*	61.4 *p.*	62.9
10	61.2 *p.*	62.7 *p.*	61.4 *p.*	62.9
11	61.2 *p.*	62.7	61.4 *p.*	62.9
12	61.2 *p.*	62.7	61.4 *p.*	62.9

N. B. The letter *p*, placed after ſome of the figures, ſignifies, *a little more*, and the letter *m*, *a little leſs*. Theſe letters were added when the exceſs or defect did not amount to more than the $\frac{1}{20}$ of a degree.

The cylinders being removed, and the water a little agitated, the general temperature in R was — 61.5 *p*.

In L — — 61.7 *p*.

The air in the room was a little
leſs than — 61.

The reader will perceive that, in the courſe of theſe experiments, the temperature of the air in the room was ſo much increaſed, as to be nearly equal, at the end of 12 or 14 minutes, to the temperature of the water in the tinned veſſels.

This was effected by the heat of the perſons who were preſent, and by candles that were brought into the room at the beginning of the experiment.

If the temperature of the air in the room were not increaſed nearly in proportion to that of the water, the difference between the heats imparted by the dephlogiſticated and atmoſpherical air, would gradually diminiſh in the courſe of the experiment, and would at length entirely vaniſh. For though the tinned veſſels were enveloped with coverings that were ſlow conductors of heat, yet when the air in the room had a lower temperature than the water in which the cylinders were immerſed, the eſcape of the heat could not be entirely prevented; becauſe we do not know any ſubſtance in nature which is altogether impervious to that principle: and hence it is manifeſt that if the portions of water, to which the airs in the cylinders imparted their heat, were placed in a cooling medium, the fluid in the veſſel, which had the higheſt temperature, would moſt ſpeedily give off its heat to the ſurrounding bodies. The exceſs of the heat communicated by the pure air, above that communicated by the atmoſpherical, would therefore gradually diminiſh, and would at length vaniſh.

It is evident that, in the foregoing experiments, moſt of the cauſes of error formerly enumerated, were avoided.

The effects of moiſture were obviated by making the cylindrical veſſels perfectly dry, and introducing the airs into them at a low temperature; as the two kinds of air were expoſed for the ſame length of time to the action of boiling water, they muſt neceſſarily have acquired the ſame degree of heat; being raiſed from the bath, and introduced into the water at the ſame moment, they muſt have been immerſed with equal velocities; the portions of water in the tinned veſſels, previouſly to the immerſion, had nearly the ſame temperature with the air in the room; and the heats imparted to them were indicated by accurate thermometers, which were very ſenſible, and had a large ſcale. After the cylinders were immerſed, the temperature of the air in the room was gradually augmented, in ſuch a manner, as to be nearly equal to the temperature of the water at the end of the experiment; and hence the errors which might proceed from an inequality in the refrigeration of the water, were in a great meaſure avoided. The fallacy ariſing from the heats imparted by the veſſels, in which the airs were contained, was alſo obviated by this apparatus. For as the cylinders were perfectly equal in temperature, weight, and capacity, the portions of heat communicated by them to the water, muſt have been equal, and muſt therefore have balanced each other. Whence it is evident that the difference obſerved, in the above experiments, muſt be attributed ſolely to the difference between the heats imparted by the dephlogiſticated and atmoſpherical air.

The following experiment proves, that if atmoſpherical air be contained in both cylinders, and if, at the beginning of the experiment, the water in one of the tinned veſſels have a higher temperature than that in the other by $\frac{2}{10}$ of a degree, this difference will exiſt during the ſpace of 12 minutes, and will appear at laſt in the water after the cylinders are removed.

EXPERIMENT IV.

Atmoſpherical air was introduced into both cylinders.

The air in the room was - 59.5,
The water in the veſſel R - 60.4,
that in L - - 60.6.

The cylinders, heated as before, were ſuddenly immerſed in the water, and the temperatures at the centre and ſurface were examined during the ſpace of 12 minutes.

	R	L	R	L
Min.	Centre.	Centre.	Surface.	Surface.
In 1	61.6 *p.*	61.7	63.9 *m.*	63.9
2	61.9 *m.*	62	63.9 *m.*	63.9
3	61.9	62 *p.*	63.9 *m.*	63.9
4	61.9	62.1 *m.*	63.9 *m.*	63.9
5	61.9	62.1 *m.*	63.8 *p.*	63.9
6	61.9	62.1 *m.*	63.8	63.9
7	61.9	62.1 *m.*	63.8	63.9
8	61.9	62.1	63.7 *p.*	63.9 *m.*
9	61.9	62.1	63.7	63.9 *m.*
10	61.9	62.1 *p.*	63.6 *p.*	63.8 *p.*
11	61.9 *p.*	62.1 *p.*	63.6 *p.*	63.8 *p.*
12	61.9 *p.*	62.1 *p.*	63.6 *p.*	63.8.

The cylinders being removed, and the water agitated, the general temperature in R was — — 62.3,

that in L was — 62.5.

At the concluſion of the experiment, the temperature of the air in the room was nearly a degree lower than that of the water in the tinned veſſels.

The reader will perceive that, in the ſecond experiment, the temperature of the water, before the cylinders were immerſed in it, was a little leſs than that of the air in the room; in the third its temperature was a little greater; and in the fourth the water had not only a higher temperature, at the beginning of the experiment, than the ſurrounding air; but the heat of the latter, at the concluſion, was nearly a degree below that of the former.

Theſe ſeem to have been the principal reaſons, why, in the ſecond experiment, more heat was imparted to the water by the cylinders, than in the third, and in the third more than in the fourth.

It is, however, manifeſt that the quantity of heat communicated muſt in ſome meaſure depend upon the velocity with which the cylinders are immerſed in the water, and it is difficult, in different experiments, to immerſe them with velocities that are preciſely equal.

In making theſe experiments, it is neceſſary that the cylinders, when they are raiſed from the water bath, ſhould be moved as ſpeedily through the air as poſſible; and at the ſame time, as ſoon as they have approached to the water, in the tinned veſſels, they ſhould be introduced with a moderate degree of velocity; for if the velocity with which they are introduced be very great, the impulſe given to the water will be ſuch as to force a part of it from the veſſels, and the warmer and colder particles at the ſurface and centre being intermixed, will produce an irregularity in the heats indicated by the thermometers.

It appears from the above experiments, that when dephlogiſticated air was contained in the cylinder L, and atmoſpherical air in R, the former communicated a greater quantity of heat to an equal bulk of water than the latter, by $\frac{2}{10}$ of a degree; and that the purity of the dephlogiſticated air, which produced this effect, was ſuch that one meaſure of it, mixed with two of nitrous air, occupied the ſpace of one meaſure.

As theſe experiments are of a very delicate nature, in order ſtill farther to guard againſt fallacy, they were reverſed by introducing dephlogiſticated air, nearly of the ſame purity with the above, into the cylinder R, and atmoſpherical air into L. It was then found, all other circumſtances being the ſame as in the former experiments, that the pure air in the cylinder R imparted more heat to the water, than the atmoſpherical air in L, by $\frac{2}{10}$ of a degree.

I varied this experiment, by introducing hot oil inſtead of boiling water into the veſſel uſed for heating the cylinders; and having thus raiſed the cylinders, one of which contained pure, and the other common air, to 300, I ſuddenly immerſed them in equal portions of water, that had nearly the temperature of the air in the room; and the thermometers being accurately examined by my friend, Dr. Percival, during ſeveral minutes, the difference of the heats, imparted by the pure and common air, was found to be nearly $\frac{3}{10}$ of a degree.

The tinned veſſels, in which the water was contained, were larger than thoſe uſed in the preceding experiments, the quantity of water in each being 32 ounces, 6 drams, and 28 grains Avoirdupoiſe. Though this experiment was made with great care, yet it was ſuppoſed that the reſult was not altogether accurate, becauſe, upon immerſing the cylinders in the water, a hiſſing noiſe was heard, and a ſmall quantity of vapour was obſerved to aſcend from each veſſel. It does not however appear, that the inaccuracy produced by the aſcent of the vapour in this inſtance was conſiderable; for if a juſt allowance be made for the heat which eſcaped from the cylinders, during their paſſage through the air, when they were raiſed from the bath, for the purpoſe of introducing them into the tinned veſſels, the ratio of the heats of pure and of common air, as determined by this experiment, will appear to be very nearly the ſame with that determined by thoſe which were formerly recited.

Though the reſults of the foregoing experiments appear to be deciſive, yet in order to aſcertain the comparative heats of the aërial fluids with greater preciſion, it was thought proper, in the experiments which I ſhall now relate, to ſubſtitute oil as a ſtandard inſtead of water; the former having a leſs capacity for heat, and conſequently having its temperature more augmented by the addition of a given quantity of that principle, than the latter.

It is proper to obſerve, that when experiments, ſimilar to thoſe recited above, are made with oil, the variations of heat produced by the immerſion of the cylinders, continue for a much greater length of time, than when they are made with water. The reader will perceive that theſe variations could not be exactly aſcertained, by placing the thermometers in the middle point between the braſs cylinders, and the interior ſurfaces of the tinned veſſels; for, in that caſe, thoſe inſtruments would not indicate equal changes of heat, unleſs they poſſeſſed equal ſenſibilities; but four thermometers equally ſenſible can ſcarcely be obtained; for, in order that they ſhould poſſeſs this quality, it is neceſſary that the quantities of mercury which they contain, and the thickneſs of their bulbs, ſhould be preciſely equal.

Now it is manifeſt that thermometers, unequally influenced by heat, cannot determine variable differences with preciſion, unleſs

leſs they be adjuſted at diſtances from the heating bodies, proportional to their ſenſibilities. This adjuſtment was accompliſhed by repeatedly introducing the heated cylinders, containing equal quantities of atmoſpherical air, into the cold oil, and varying the diſtances of the thermometers, until the heats indicated by them were equal. After many trials made with the moſt ſolicitous attention, in which I was aſſiſted by my friend, Dr. Hamilton, on whoſe accuracy I could place the greateſt reliance, the central thermometers were at length adjuſted with ſuch exactneſs, that the heats indicated by them did not vary from each other more than the tenth of a degree, which will appear from the following experiment.

EXPERIMENT VI.

Air in the room 56½.

One pound, four ounces and ¾ of an ounce Avoirdupoiſe of ſpermaceti oil, was introduced into each of the tinned veſſels, the depth of the oil being a little more than half the height of the veſſel, in conſequence of which the central thermometers reached to a ſmall diſtance below the ſurface of the oil, and were therefore compleatly immerſed in it.

The temperature of the oil was a few degrees lower than that of the air in the room, in order that when it was heated by the cylinders, its mean heat might be nearly equal with that of the ſurrounding air. The portions of oil in the tinned veſſels were equally agitated by ſmall glaſs tubes, that the heat might be equally diffuſed over them; the agitation being continued for a few minutes until the thermometers became ſtationary. This was repeated ſeveral times, and the temperatures being obſerved, immediately before the cylinders were immerſed, that in R was found to be 53.5; L 53.5.

The cylinders containing atmoſpherical air were heated in an adjoining room, by means of the water bath formerly deſcribed, which was kept at the boiling point for 10 minutes. The bath and cylinders being then removed into the room, in which the tinned veſſels were placed, and the cylinders being ſpeedily immerſed in the oil, the temperatures, during the ſpace of five minutes, were as follows:

Centre.

	Centre.		Surface.	
	R	L	R	L
1 Min.	53.6	53.6	58.4	60
2	54.3	54.4	60.7	61.3
3	54.8	54.7	61.1	61.3
4	55.3	55.2	61.1	61.3
5	55.8 *m.*	55.7	61.1 *p.*	61.3.

The air in the room at the end of the experiment was 60. As the two thermometers in each of the tin veſſels were placed on the ſame ivory plate, having a double ſcale, the reader will perceive that when thoſe at the centre were adjuſted in ſuch a manner as to indicate nearly equal heats, the ſurperficial ones could not be ſo adjuſted, unleſs the latter had poſſeſſed preciſely the ſame degree of ſenſibility with the former; but as their ſenſibilities were different, the heats indicated at the ſurface, during the firſt two minutes, were very unequal; after which they approached to each other within the .2 of a degree.

I then made the following experiment, with the aſſiſtance of Dr. Hamilton, in the preſence of my learned friend, Mr. Kirwan, and of the late much lamented Doctors Watkinſon and Kerr.

E x-

EXPERIMENT VII.

The air in the room being 54, and the oil in R 51.3 plus; that in L 51.3; dephlogiſticated air being introduced into R, and air phlogiſticated by iron filings and brimſtone into L; and the cylinders being heated as before, and immerſed in the oil; the temperatures obſerved by Dr. Hamilton, during ſeven minutes, were as follows:

	Centre.		Surface.	
	R	L	R	L
2 Min.	52.1	52.3		
3½ nearly	53.4	53.1	59.5	59.7
5	54.4	54	59.5	59.5
6	54.8	54.4	59.5	59.5
7	55.1	54.7 plus	59.4	59.4

The purity of the dephlogiſticated air was examined at the concluſion of this experiment, a part of it being drawn from the cylinder by means of an exhauſted glaſs veſſel, when it was found that one meaſure of it, mixed with two of nitrous air, occupied the ſpace of a little more than one meaſure. The phlogiſticated air, in the courſe of the experiment, was nearly changed into atmoſpherical air.

This experiment was repeated with the following reſult.

EXPERIMENT VIII.

Air in the room 54.5.

Dephlogiſticated air, nearly as pure as that uſed in the foregoing experiment, was introduced into the cylinder R, and common air into L.

The temperature of the oil in the veſſel R was 51.3; that in L 51.1.

The cylinders being heated and immerſed in the oil, the temperatures, indicated by the central and ſuperficial thermometers, were,

	R	R	L	L
Minutes.	Centre.	Centre.	Surface.	Surface.
in 2	51.8	51.7		
3	52.6	52.3 *p.*		
4	53.6	53.2	58.8	58.9 *p.*
5	54.	53.5	58.8	58.8
6	54.3	53.8	58.7 *p.*	58.7 *p.*
7	54.5	54.2	58.7	58.7

In Experiment VI. the heat indicated by the central thermometer in the veſſel R, at the end of five minutes, exceeded that in the veſſel L by $\frac{1}{10}$ of a degree.

In Experiments VIIth and VIIIth the exceſs was a little leſs than $\frac{4}{10}$ of a degree, allowance being made for the difference of temperature indicated by the thermometers, in the veſſels R and L, before the cylinders were immerſed in them.

Theſe experiments were frequently repeated with great care, and it was uniformly found, that when dephlogiſticated air, which had the purity of 1, as indicated by Dr. Prieſtley's Teſt, was introduced into R, and atmoſpherical air into L, the heat imparted by the former to the oil, at the end of four minutes, was $\frac{3}{10}$ of a degree greater than that by the latter; the variation in the reſults of different trials being not more than the $\frac{1}{20}$ of a degree.

I next reverſed the experiments, with pure and phlogiſticated air, in the preſence of Mr. De Luc, pure air being introduced into the cylinder L, and phlogiſticated air into R. But in order that the reſults might be more deciſive, the accuracy of the apparatus was firſt tried, by making the experiment with atmoſpherical air, in both cylinders, when it was found that the heats communicated to the central thermometers, did not vary from each other more than the $\frac{1}{20}$ of a degree.

This will appear from the following experiment, in which the variations of temperature were obſerved by Mr. De Luc, during the ſpace of ten minutes.

EXPERIMENT IX.

Air in the room	—	54
The oil in the tinned veſſel R		52.1
That in L —	—	52.3

The cylinders, containing atmoſpherical air, being heated as before, and immerſed in the oil,

Minutes.		Centre.		Centre.
in 2	R was	53.3	L	53.5
4		54.5 *p.*		54.7
5		54.9 *p.*		55.1
6		55.1 *p.*		55.2 *p.*
8		55.4 *p.*		55.6 *p.*
9		55.5 *p.*		55.7

In this experiment, therefore, the difference of the heats communicated to the central thermometers did not exceed the $\frac{1}{20}$ of a degree; allowance being made for the exceſs of temperature in the veſſel L, previouſly to the immerſion of the cylinders; and during the greater part of the time the heats communicated were perfectly equal.

EXPERIMENT X.

Air in the room	—	58
The oil in the veſſel R	—	56.9
——— in L	—	56.9

Dephlogiſticated air being introduced into the cylinder L, and phlogiſticated air into R; and the cylinders being heated and immerſed in the oil, the temperatures, as obſerved by Mr. De Luc, during 10 minutes, were as follows:

Minute.	Centre.	Centre.
1	R 57.2 *p.*	L 57.2 *p.*
2	57.7	57.9 *p.*
3	58.3 *p.*	58.6
4	58.9	59.2
5	59.3	59.5
6	59.7	59.9
7	59.9 *p.*	60.1 *p.*
9	60.1	60.3
10	60.3	60.4 *p.*

It is proper to obſerve, that the heats indicated by the thermometers at the ſurface in this experiment, were nearly equal; and the ſame thing happened in Experiment VII.

That it might be known, whether the airs ſuffered any change by expoſure to heat in the cylinders, their purity was examined previouſly to the experiment, in the preſence of Mr. De Luc, when it appeared that one meaſure of the dephlogiſticated air, mixed with two of nitrous air, occupied the ſpace of $\frac{3}{10}$ of a meaſure; and two meaſures of the phlogiſticated air, mixed with one of nitrous air, occupied the ſpace of 2.6. Being again examined, at the concluſion of the experiment, in the preſence of the ſame philoſopher, the purity of the dephlogiſticated air was found to be ſo much diminiſhed, that one meaſure of it, mixed with two of nitrous air, occupied the ſpace of $\frac{8}{10}$ of a meaſure; and the phlogiſticated air was nearly changed into atmoſpherical, two meaſures of it, with one of nitrous air, occupying the ſpace of 2.15.

It appeared from ſubſequent trials, that the ſtop cocks, which were ſcrewed to the ends of the cylinders, when expoſed to heat, were not altogether air tight: for though they were made with ſuch accuracy, that the cylinders, when cold, could be very nearly exhauſted, yet when the latter were placed in the water bath, the heat which aſcended along the ſtems, was not ſo ſpeedily received by the ſtoppers, as by the cavities to which they were adapted; and hence the latter, expanding more ſpeedily than the former, the excluſion of the external air became leſs perfect.

It appears from this experiment, that phlogiſticated air has a much greater tendency to mix with the air of the atmoſphere than pure air. For the difference of alteration, in thoſe ſpecies's of air, did not ariſe from a difference in the ſtop cocks, becauſe the ſame effects were produced when the experiment was reverſed. The truth of this obſervation was moreover confirmed, by expoſing pure and phlogiſticated air to a moderate heat, in thin bladders, that had been previouſly well rubbed.

In the above experiment, the heat obſerved in the centre of the veſſel L, at the end of four minutes, was nearly $\frac{3}{10}$ of a degree greater than that obſerved in R. After the 4th minute was expired, the difference began to diminiſh, and in the courſe of the ſix ſucceeding minutes it nearly vaniſhed, the exceſs of heat in L, at the end of that time, being only $\frac{1}{10}$ of a degree.

It was before obſerved, that, in the above experiments, the heats indicated by the ſuperficial thermometers were nearly equal; the temperature of the oil, at the ſurface, being liable to be influenced by cauſes which could not with certainty be counteracted. The chief of theſe ſeemed to be, the tendency of the heated particles of oil to aſcend to the ſurface; the facility with which thoſe particles communicate their heat to the neighbouring bodies; the agitation given to the oil by the immerſion of the cylinders; the heat imparted by the ſtems which connect the cylinders with the croſs bars; the influence of the external air; and the difficulty of adjuſting the ſuperficial thermometers with ſuch preciſion that they ſhould indicate equal heats. But at the centre, the exceſs of heat communicated by the dephlogiſticated air, whether it were contained in the veſſel L or R, was very nearly invariable, as the experiment, when repeated with accuracy, never failed to ſucceed within the $\frac{1}{20}$ of a degree; and when water was uſed as a ſtandard, inſtead of oil, the exceſs of heat imparted by the pure air appeared not only at the ſurface and centre, but was obſerved to exiſt after the cylinders were removed, and the ſeveral parts of the water were reduced by agitation to a common temperature.

It is proper to remark, that the difference of the heats communicated by dephlogiſticated air to oil and water, as indicated by the central thermometers in theſe experiments, was leſs than might have been expected from the difference of the capacities of thoſe ſubſtances. For the capacity of water is to that of ſpermaceti oil nearly as 2 to 1, from which it might be ſuppoſed, that the heats imparted would be as one to two. But they appear, in the foregoing experiments, to have been as 2 to 3. We have alſo ſeen, that the difference of heat at the centre, when the experiment is made in oil, vaniſhes more ſpeedily than when it is made in water. Theſe irregularities ſeem principally to be occaſioned by the difference in the conducting powers of thoſe ſubſtances with reſpect to heat, and by the greater tendency of the particles of warm oil to aſcend, in conſequence of which the heat is ſuddenly accumulated at the ſurface, and is ſpeedily communicated to the neigh-

bouring

bouring bodies. It is manifeſt, that the cauſes which diminiſh the differences of heat at the ſurface, will gradually produce a ſimilar diminution at the centre.

When the experiment was made by introducing the cylinders into the tinned veſſels, which contained atmoſpherical air alone, the effects produced by the ſudden tranſmiſſion of the heat were more evident; for, in that caſe, the difference of the heats communicated by pure and phlogiſticated air could not at all be perceived at the ſurface, and at the centre it did not appear till after the expiration of many minutes; and though, at length, pure air evidently communicated more heat than phlogiſticated air, yet the reſults of the different trials were ſo irregular, that no preciſe concluſions could be drawn from them.

The foregoing experiments prove, in general, that pure air communicates more heat to an equal bulk of oil or water than atmoſpherical air.

I next endeavoured to determine the comparative quantities of heat communicated by phlogiſticated, inflammable, and fixed air.

From the preceding experiments on pure and atmoſpherical air, it appears, that when the cylinders are immerſed in oil, more minute differences of heat are indicated by the central thermometers, than when they are immerſed in water: for this reaſon, in the experiments which follow, oil was uſed as a ſtandard. It is of great importance to the ſucceſs of the experiment, when this fluid is made the ſtandard, that the heat, previouſly to the immerſion of the cylinders, ſhould be uniformly diffuſed over the oil, and ſhould remain as nearly ſtationary as poſſible. But the heat of the oil cannot remain ſtationary, if the temperature of the air in the room vary; and in rooms which are expoſed to the influence of the weather, a permanent temperature can ſcarcely be obtained. For the purpoſe of avoiding this cauſe of inaccuracy, the apparatus was placed in a vault below the ſurface of the earth, where the alterations of temperature were very ſlow, and the diffuſion of heat nearly uniform.

It was before obſerved, that as the ſtop-cocks, which were fixed to the ſtems of the cylinders, were unequally heated, they did not ſufficiently interrupt the communication between the external air and that in the cylinders; in conſequence of which the air was found to undergo a conſiderable change in the courſe of the experiment. This difficulty was obviated in the following manner: as ſoon as the air was introduced into the cylinder, the ſtop-cock was unſcrewed from the extremity of the ſtem, and the cavity of the latter was accurately cloſed by a cork, after which the cock was again fixed to it. In conſequence of this, when the apparatus was heated, the cork, by its elaſticity, adapted itſelf to the increaſed capacity of the ſtem; and thus the interruption of the communication between the air in the cylinder and the external air, being rendered nearly complete, the former was found to ſuffer very little change from expoſure to heat.

With theſe precautions I endeavoured to determine the capacity of phlogiſticated air, the apparatus having been previouſly ſo adjuſted, that when atmoſpherical air was contained in both cylinders, the heats indicated by the central thermometers were very nearly equal, as appears from the experiment which follows.

EXPERIMENT XI.

Air in the vault — 60.7

One pound, three ounces and two drams of ſpermaceti oil being introduced into each of the tinned veſſels, the temperature in the veſſel R, was 59.9; that in L, 60 plus.

The cylinders, containing atmoſpherical air, were heated in an adjoining vault by means of the water bath, and were ſuddenly immerſed in the oil, upon which the central heats being accurately marked during the ſpace of 10 minutes,

Minutes.

In 2	R was 60.2		L 60.2	
3	60.9		61.	
4	61.5		61.5	plus.
5	62.	minus.	62.1	minus.
6	62.3	plus.	62.4	plus.
7	62.6	minus.	62.7	
8	62.8		62.9	
9	62.9		63.	
10	63.	plus.	63.1	plus.

Air in the vault 61.7.

E x-

EXPERIMENT XII.

Air in the vault	—	62.6
Oil in the tinned veſſel R	—	62.1
In the veſſel L	—	62.2

Air phlogiſticated by hepar ſulphuris was introduced into the cylinder R, and atmoſpherical air into L. The cavities of the ſtems of the cylinders were accurately cloſed by corks in the manner deſcribed above; the cylinders were then heated as before, and immerſed in the oil; and the central heats being obſerved during ſix minutes,

Minute.		
In 1	R was 62.2	L 62.3
2	62.5	62.6 *p.*
3	63.1 *p.*	63.3
4	63.7 *p.*	63.9 *p.*
5	64.2 *p.*	64.3 *p.*
6	64.5	64.6

Air in the vault at the concluſion of the experiment 63.3.

This experiment was repeated with the following reſult.

E x-

EXPERIMENT XIII.

Air in the vault	—	63.
Oil in the veſſel R	—	62.3 *m.*
In L	— —	62.3 *m.*

The cylinders, containing phlogiſticated and atmoſpherical air as before, were heated and immerſed in the oil, and the central heats being obſerved during eight minutes,

	R	L
In 1 Minute	62.4	62.4
2	62.7 *p.*	62.7 *p.*
3	63.4	63.5
4	64. *p.*	64.1
5	64.5 *m.*	64.5
6	64.8 *p.*	64.9
7	65.1	65.1 *p.*
8	65.3 *p.*	65.3

Two meaſures of the phlogiſticated air, uſed in theſe experiments, having been mixed, previouſly to its introduction into the cylinder, with one of nitrous air, the meaſures of the teſt were 2.9. After the experiments were finiſhed, a part of it was drawn from the cylinder, and being again examined by nitrous air, the meaſures of the teſt were found to be 2.8, very nearly. From which it appears, that very little alteration had been produced in it by expoſure to heat in thoſe experiments.

An interval of ſeveral days had elapſed between each of the experiments now recited. They were made in the month of July 1783, when the heat of the ſun was gradually increaſing, and at length became very intenſe. This accounts for the alteration in the temperature of the vault. It was found, in the courſe of eleven days, to ſuffer a change of 3 degrees.

From theſe experiments, compared with many others of a ſimilar kind, which it is unneceſſary to relate, it appears, that the difference of the central heats, imparted to the oil by atmoſpherical and phlogiſticated air, was nearly $\frac{1}{10}$ of a degree.

Inflammable and fixed air were next examined, with great care, by the ſame apparatus. From the reſults of a variety of trials, compared with each other, it manifeſtly appeared, that the central heat communicated to the oil, by fixed air, was a little leſs than that communicated by atmoſpherical air, the difference being not ſo great as $\frac{1}{10}$ of a degree;

gree; and, on the contrary, when inflammable and atmoſpherical air were compared, the central heat imparted by the former was a little greater than that by the latter, the difference being, in this inſtance, alſo, leſs than the $\frac{1}{10}$ of a degree.

It may perhaps be doubted, whether ſuch minute differences may not be liable to much uncertainty. With reſpect to this I would obſerve, that the $\frac{1}{10}$ of a degree on the ſcale of the thermometers, uſed in my experiments, is nearly as diſtinct as an intire degree of the thermometers that are commonly made with Farhenheit's ſcale. And as each diviſion can be nearly biſſected by the eye, the $\frac{1}{20}$ of a degree may be diſtinguiſhed with conſiderable preciſion. For this reaſon, I have ventured to lay the reſults of the foregoing experiments with inflammable, fixed, and atmoſpherical air, before the reader. From numerous trials, made with the utmoſt care, and variouſly diverſified, the abovementioned differences appeared to exiſt. There is, however, deciſive evidence for believing, that the real differences do not exceed thoſe which have been determined by theſe experiments, becauſe the reſults of the ſeveral trials correſponded within the $\frac{1}{20}$ of a degree.

If, however, we ſhould ſuppoſe, that the heats imparted by equal bulks of theſe three ſpecies of air were perfectly equal, it would not affect the doctrine contained in the following pages.

In the experiments recited above I have endeavoured to determine the *differences* of the heats imparted to water by fixed, phlogiſticated, inflammable, atmoſpherical and dephlogiſticated air. That the *entire* heats communicated by theſe fluids might be known, it was neceſſary to diſcover the quantity which was yielded by the cylinders employed for the purpoſe of immerſing them in the water. This was determined by the experiments which follow.

EXPERIMENT XIV.

Air in the room —	60.5	
Water in the tinned veſſel R	60.	*m.*
In L — —	60.	*m.*

The braſs cylinders, containing atmoſpherical air, being heated as before, and immerſed in the tinned veſſels, were ſuffered to remain during the ſpace of eight minutes; after which, being removed, and the water in both veſſels being equally agitated, the temperature in R was found to be 62.2; that in L 62.2 *m*.

It appears, therefore, that when atmoſpherical air was in both cylinders, the heats imparted were nearly equal, the difference being not greater than the $\frac{1}{20}$ of a degree.

The quantity of water contained in each of the veſſels was 20 ounces Troy.

In order to diſcover the quantity of heat, communicated by the air in this experiment, the water was ſuffered to enter into one of the cylinders in the following manner: the bottom of the cylinder R being opened, the ſpherical plate which cloſed it was introduced laterally, in ſuch a manner, that the ſemicircle, dividing its ſurface into two equal parts, ſhould come into contact with the half of the circular extremity of the cylinder, to which it was faſtened by ſolder. Hence the remaining half of the bottom being open, the water, when the cylinder was immerſed in it, was ſuffered to enter freely, and the air at the ſame time was permitted to eſcape by the cock which was faſtened to the upper extremity of the ſtem. In order that the cylinder, when its bottom was opened, might be compleatly ſurrounded by the water in the tinned veſſel, it was neceſſary that the quantity of this fluid ſhould be twice as great as that which was required to cover it when the bottom was cloſed, the water in the former caſe being permitted to enter into the cylinder, and in the latter being excluded. For this reaſon, in the following experiment, 40 ounces Troy of water were introduced into the tinned veſſel R, and 20 ounces into L.

EXPERIMENT XV.

Air in the room	—	62.
The temperature of the water in the veſſel R, was		61.7 plus.
That in L	—	61.8

The cylinders L and R, the latter being open above and below, and the former containing atmoſpherical air, were heated as in the foregoing experiment, and immerſed in the water, where they were ſuffered to remain for eight minutes; being then removed, and the water in both veſſels being equally agitated, the general temperature in R was found to be 62.7 *p*; that in L 64.

In this experiment the heat imparted by the cylinder R was one degree; that by L 2.2, the quantity of water in the veſſel R being 40 ounces, and that in L 20. If the quantity of water in the veſſel R had been only 20 ounces, the heat communicated by the cylinder alone would have been nearly two degrees; and the heat imparted to the water in the veſſel L, by the cylinder filled with atmoſpherical air being 2.2, the difference gives nearly $\frac{2}{10}$ of a degree for the heat yielded by the air *.

In the above experiment the utmoſt attention was given to avoid the cauſes of fallacy, by which I had been deceived in the experiments on the capacities of the aërial fluids, recited in my former publication.

It muſt be obſerved, that thoſe experiments were made under very unfavourable circumſtances. They were my firſt attempts towards the diſcovery of truth in an obſcure and intricate part of ſcience, where the ſources of error were numerous, and the means of avoiding them had not been aſcertained. Differences, which aroſe from the condenſation of greater or leſs quantities of moiſture in the bladders that were employed in my experiments, I attributed to the different powers of the aërial fluids for communicating heat. My firſt experiment with atmoſpherical air was made with a freſh bladder, which is more diſpoſed to attract moiſture, and is dried with greater difficulty, than thoſe which have been previouſly freed from their impurities by waſhing. This experiment was again repeated, after a conſiderable interval of time had elapſed, and the ſame bladder which had been accidentally purified from its ſalts and mucilage, by being kept for a conſiderable time under water, in the previous experiments, was again dried, and was uſed for the purpoſe of determining the heat imparted by fixed air. The thermometers which I employed were not ſufficiently ſenſible, nor were their ſcales of ſuch a ſize as to indicate minute variations of heat; and the immerſion of the bladder and thermometer in the water being regulated by the hand, their relative poſitions could not be adjuſted with the accuracy which ſuch delicate experiments required.

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* The reader will ſee the heat imparted by atmoſpherical air more accurately ſtated in page 234.

The heat imparted by the bladder was examined by rolling it with packthread, raiſing it to the deſired temperature, and immerſing it in water a little below the heat of the air in the room. I was not then aware, that, in theſe circumſtances, a ſmall portion of the ſurface of the bladder being in contact with the water, it communicates its heat more ſlowly than when it is diſtended with air.

The dephlogiſticated air employed in the experiment, recited in my former publication, was obtained from red lead moiſtened with yellow ſpirit of nitre, which was expoſed to heat in a Florentine flaſk, having a flaccid bladder, that was previouſly freed from air, tied to its neck. As it was my firſt attempt to procure dephlogiſticated air, I had not obſerved that, in theſe circumſtances, this fluid muſt be mixed with a conſiderable quantity of aqueous and nitrous vapour. I, therefore, did not deem it neceſſary, previouſly to the experiment, to paſs the air through water, and transfer it into a different bladder; in conſequence of which, when I attempted to determine its abſolute heat, I was led into an error of nearly two degrees and a half.

Having determined the entire quantity of heat, yielded by atmoſpherical air, I next endeavoured to diſcover the heat which was loſt by the cylinders, when they were raiſed from the bath, for the purpoſe of immerſing them in the water. It was formerly obſerved, that in making the experiments recited above, the water bath, with the cylinders contained in it, was placed near to the bottom of the table on which the upright bar and tinned veſſels were fixed; and that the cylinders, being raiſed from the bath, were directed into the veſſel by means of the bar and ſocket. During this operation they were made unavoidably to paſs through a portion of cold air, and from the ſlightneſs of the cylinders, and the facility with which they tranſmitted heat, it is manifeſt, that the quantity imparted to the air muſt have been conſiderable. The heat which was thus loſt was eſtimated by the experiment which follows:

Experiment XVI.

Air in the room — 63.2
Twenty ounces Troy of water was introduced into the tinned veſſel L at — 61.9 *p.*

The cylinder L, containing atmoſpherical air, was expoſed, as in former ſimilar experiments, to a boiling heat in the water bath for 10 minutes.

The bath and cylinders were then placed on the ſame table with the tinned veſſel L, in which the cold water was contained, a deal board being interpoſed between them to prevent the paſſage of the heat from the bath to the veſſel, upon which the cylinder was inſtantly introduced into the veſſel, and the bath removed: the cylinder being ſuffered to remain in this ſituation during ſix minutes, it was then raiſed from the veſſel; the water was ſlightly agitated, and the general temperature at the end of

7 minutes was — 64.5 *m.*
8 — — 64.5 *m.*
9 — — 64.5 *m.*

The air in the room at the
conclufion of the experiment was — 63.3

Hence the heat communicated to the water was 2.5 degrees.

In this experiment the cylinder, previouſly to its immerſion in the water, came into contact with a very ſmall quantity of cold air, and was made to paſs through it with as much velocity as poſſible. Though theſe precautions were uſed to diminiſh the eſcape of heat from the cylinder, during its paſſage through the air, yet it was impoſſible entirely to prevent it. If we ſuppoſe that in the laſt experiment a quantity of heat eſcaped, which would have raiſed the water $\frac{1}{10}$ of a degree, we ſhall have 2.6 degrees for the whole of the heat communicated by the cylinder, and the air which it contained.

In Experiments XIV. and XV. the heat communicated to the water by the cylinder L, was only 2.2 degrees. In thoſe experiments, therefore, the cylinder muſt have loſt nearly $\frac{1}{6}$ of its heat during its paſſage through the air.

In Experiment XV. the cylinder R, having remained in the water bath for 10 minutes,

nutes, was raiſed to the temperature of 200, as appeared from the thermometer introduced into its centre.

The heat of the water in the tinned veſſel, previouſly to the immerſion of the cylinder, was 61.7. Hence the difference between the temperature of the water and cylinder was 138.3; of this quantity nearly $\frac{1}{6}$ was carried off by the external air. The remainder, or 115, plus 61.7, gives the true temperature of the cylinder previouſly to its immerſion in the water. The temperature of the water, at the end of eight minutes, when the cylinder was removed from it, was 62.7 nearly. The cylinder was therefore cooled by the water 114 degrees, and the water heated by the cylinder one degree.

The weight of the water in the tinned veſſel was 40 ounces Troy, and the capacity of the veſſel for receiving heat was equal to that of one ounce of water plus, .23 of an ounce. The water and veſſel were, therefore, equal in capacity to 41.23 ounces of water.

The heat, ſeparated from the cylinder, was 114.25 degrees; and that received by the water was one degree, conſequently the capacity of the water and veſſel, or of 41.23

ounces of water, was to that of the cylinder as 114.25 to 1. If the quantity of water had been 114.25 ounces, the capacity of the cylinder would have been equal to that of one ounce of water; but the quantity of water was only 41.23 ounces. The capacity of the cylinder muſt, therefore, have been leſs than that of an ounce of water in the proportion of 41.23 to 114.25. That is, the capacity of the cylinder was equal to that of .3608 of an ounce of water.

Hence we may determine the heat, that would have been communicated by the cylinder, in the experiment in queſtion, if the tinned veſſel R had contained only 20 ounces of water, which was the quantity contained in the veſſel L. For, according to Richman's rule, when two unequal quantities of water are mixed together at different temperatures, if the products of the maſſes, multiplied into the ſeparate heats, be added together; and if this quantity be divided by the ſum of the maſſes, the quotient will give the temperature of the mixture. That is, calling the maſſes M and m, and the ſeparate heats C and c; the temperature of the mixture will be $\frac{MC + mc}{M + m}$.

It was proved, that the heat ſeparated by the water from the cylinder R, in Experiment XV. was 114.25 degrees; and the heat of the water at the end of the 8th minute was 62.7 *p*. Adding this to 114.25, we have 176.95 for the temperature of the cylinder, at the moment of its immerſion in the water. If we conceive the quantity of the water to be 20 ounces, and the veſſel to be equal to one ounce plus .23 of an ounce of water, the veſſel and water will be equal to 21.23 ounces. The temperature of the water was 61.7; and the cylinder was equal to .3608 of an ounce of water.

We have, therefore, according to Richman's rule, $\frac{21.23 + 61.7 \times .3608 + 176.95}{21.23 + .3608},$ equal to 63.626 for the temperature of the mixture; and the heat of the water, previouſly to the immerſion of the cylinder, being 61.7, the difference gives 1.926 for the heat which would have been communicated by the cylinder, if the quantity of water in the veſſel R, had been equal to that in L: that is, if it had been 20 ounces.

The heat imparted to the water in the veſſel L was 2.2 degrees. Of this quantity of heat, it appears, that 1.926 was yielded by the cylinder; from which it follows, that the heat imparted by the air was .274. It is proper to obſerve, that there was an error of $\frac{1}{20}$ of a degree in this experiment; for when it was compared with others of a ſimilar kind, the mean reſult gave .23 of a degree for the heat communicated by atmoſpherical air.

To diſcover from theſe experiments the relation, which the abſolute heat of atmoſpherical air bears to that of water, we may conſider what would have been the effect produced if this ſpecies of air had contained the ſame abſolute heat with water. In that caſe, if the air were only the one hundredth part as denſe as water, and were raiſed 100 degrees above the temperature of the water, it would communicate to it nearly one degree of heat. If it were only the one eight hundredth part as denſe, and were raiſed 100 degrees above the heat of the water, it would communicate to it nearly the eighth part of a degree.

Now the denſity of atmoſpherical air is to that of water in a proportion ſomewhat leſs than that of 1 to 800; and, in the foregoing experiments, the exceſs of the temperature of the air above that of the water was 115.5 degrees. If, therefore, in the experiment in queſtion, the atmoſpherical air had contained the ſame abſolute heat with water, it would have communicated to the water nearly the eighth part of a degree; but it communicated a little more than the fifth of a degree. Atmoſpherical air muſt, therefore, contain a greater quantity of abſolute heat than water.

Dr. Irvine has pointed out a general rule, by which the comparative quantities of heat in bodies may be eſtimated, when the quantities of matter, and the changes produced in the temperatures are unequal. In that caſe the comparative quantities of heat are reciprocally as the changes in the temperatures multiplied into the quantities of matter. By the help of this rule, the ratio of the heat of atmoſpherical air, to that of water, may be more accurately calculated in the following manner.

It is evident, that as equal bulks of air and water were taken, if the denſities of the water and air had been equal, the abſolute heat of the air would have been to that of the water as 1 to 497.5, which is the reciprocal proportion of the changes produced in the temperatures.

Again, if the changes produced in the temperatures had been equal, the quantities of abſolute heat would have been reciprocally as the quantities of matter. It follows, that neither being equal, the quantities of abſolute heat are in the compound ratio of the ſenſible heat gained by the water to that ſeparated from the air, and of the quantity of water to that of air.

It was before ſhown that the veſſel and water were equal to 21.23 ounces of water; and the ſpecific gravity of water being to that of atmoſpherical air in the proportion of 830 to 1: it follows, that the quantity of matter contained in 21.23 ounces of water, is to that contained in 20 ounce meaſures of air, as 881 to 1.

In Experiment XV. the temperature of the water in the veſſel L, previouſly to the immerſion of the cylinders, was 61.8; the heat of the cylinder, with the air contained in it, was 200; the difference was 138.2; and $\frac{1}{6}$ being allowed for the heat imparted to the external air, we have 115 nearly for the exceſs of the heat of the air in the cylinder, above that of the water. The true heat of the cylinder, therefore, previouſly to immerſion, was 176.8; the temperature of the water at the end of the experiment was 64. The air was conſequently cooled by the water 112.8, and it was before ſhown that the water was heated by the air .23 of a degree. Hence the comparative heat of atmoſpherical air is to that of water in the compound ratio of 881 to 1, and of .23 to 112.8.

From which it appears, that the quantity of heat contained in the former of theſe elements, is to that contained in the latter, as 202.63 to 112.8, or as 1.79 to 1.

The comparative heat of pure air may be calculated as follows: It was before proved, that the two aërial fluids, pure and phlogiſticated air, mixed in certain proportions, compoſe the air of the atmoſphere; and that of the atmoſpherical air, it is the purer part alone which is diminiſhed by phlogiſtic proceſſes. Hence the quantity of pure air, contained in common air, may be diſcovered by obſerving the diminution which theſe proceſſes produce.

When nitrous and common air are mixed over water, in ſuch proportions as to effect a compleat ſaturation, and the mixture is ſuffered to remain for a ſufficient length of time, the whole of the nitrous air, and $\frac{1}{5}$ of the common air, diſappear. When common air is expoſed to a mixture of iron filings and brimſtone, the mean diminution, as determined by the experiments of Dr. Prieſtley and Mr. Scheele, is 27 parts of an 100. In the former inſtance it is probable, that a ſmall portion of the pure air is protected by the phlogiſticated from the action of the nitrous air, as has been remarked by Mr. Kirwan; and in the latter caſe it is likely that the whole of the pure air is ſeparated by the iron filings and brimſtone, and that a part of the phlogiſticated air alſo diſappears, as it is well known that a ſmall quantity of this fluid is capable of being abſorbed by water. If, therefore, we take the mean reſult of the experiments with nitrous air, and with iron filings and brimſtone, we ſhall have nearly 25 parts of an 100 for the true diminution. From which it appears, that nearly $\frac{1}{4}$ of the air of the atmoſphere conſiſts of dephlogiſticated air.

In Experiment II. p. 190, the purity of the dephlogiſticated air was ſuch, that one meaſure

ſure of it, mixed with two of nitrous air, occupied the ſpace of one meaſure. From Dr. Prieſtley's experiments it appears, that two parts of nitrous air ſaturate very nearly one of dephlogiſticated air; becauſe when they were mixed in this proportion, the dephlogiſticated air being very pure, not more than $\frac{3}{100}$ of a meaſure remained. See Prieſtley's Obſervations and Experiments on Air, vol. 4. ſect. 25. When, therefore, one meaſure of dephlogiſticated air, mixed with two of nitrous, occupy the ſpace of one meaſure, it follows, that the dephlogiſticated air muſt have contained two thirds of its bulk of pure air. For the quantity of air that diſappears is two meaſures. Of theſe, one part muſt have been pure air, and two parts nitrous. The quantity of pure air, therefore, contained in the dephlogiſticated air, muſt have been one third of two meaſures, or two thirds of one meaſure; and as the whole of the dephlogiſticated air, originally ſubjected to the examination, was one meaſure, it is evident that it muſt have contained two thirds of its bulk of pure air. The part which remained undiminiſhed by nitrous air, and unabſorbed by water, was phlogiſticated air. It is proper to obſerve, that the air had been placed, previouſly to its examination, a conſiderable time over water, to free it from the admixture of fixed air.

Thus it appears, that the dephlogiſticated air, which was introduced into the cylinder L, in Experiment II. p. 190, contained two thirds of its bulk of pure air, and one third of phlogiſticated air; but the common air, in the oppoſite cylinder R, contained three fourths of its bulk of phlogiſticated air, and one fourth of pure air. In that Experiment we may conceive the phlogiſticated air in L, amounting in volume to one third of the whole, to be balanced by an equal quantity of the ſame ſpecies of air in R. We may alſo conceive a portion of the pure air in L, to be balanced by a quantity of that fluid in R, amounting to one fourth of the whole; and the cylinders, being equal in weight and capacity, preciſely balanced each other.

The entire quantity of pure air in L was $\frac{2}{3}$ of the whole, the quantity in R was $\frac{1}{4}$. The exceſs of the pure air in L, above that in R, was conſequently .41, and the exceſs of the phlogiſticated air in R, above that in L, was alſo .41. The experiment in queſtion,

tion, therefore, gives us the difference between the heat imparted by .41 parts of pure air, and that imparted by an equal bulk of phlogiſticated air. This difference was $\frac{2}{10}$ of a degree. Hence we may diſcover the quantities of heat, which would have been communicated to the water, if the cylinder R had been filled with phlogiſticated air, and L with air perfectly pure. For let the heats, which would have been yielded by pure and phlogiſticated air, be called x and y,

$$\text{Then} \quad \frac{41}{100}x - \frac{41}{100}y = .2$$

$$41\;x - \frac{4100}{100}y = 20$$

$$4100\;x - 4100\;y = 2000$$

$$\text{Therefore} \quad x - y = .487.$$

But it was ſhown that the entire quantity of heat imparted to the water by the common air, in the cylinder R, was .23; and this ſpecies of air contains $\frac{1}{4}$ of its bulk of pure air.

$$\text{Therefore} \quad \tfrac{1}{4}x + \tfrac{3}{4}y = .23$$

$$x + \frac{12}{4}y = .92$$

$$4\;x + 12\;y = 3.68$$

$$\text{and} \quad x - y = .487$$

$$12\;x - 12\;y = 5.844$$

$$\text{but} \quad 4\;x + 12\;y = 3.68$$

there-

therefore $16\,x = 9.524$

and $x = .595$

but $x - y = .487$

therefore $y = .108$

Whence it appears that the entire quantity of heat, which would have been yielded by pure dephlogiſticated air, in the experiment in queſtion, is .595; and that the quantity which would have been imparted by air compleatly phlogiſticated, is .108.

It was before obſerved, that as the temperature of the air in the room, in Experiments II. and III. was gradually encreaſed in proportion to the heat received by the water in the tinned veſſels, the error ariſing from the irregularity of cooling was in a great meaſure avoided. But as the quantities of heat received by the water were unequal, it was impoſſible entirely to obviate this ſource of fallacy: for the air having nearly the ſame degree of heat with the water in the veſſel which had the higher temperature, it is manifeſt, that the water in that which had the lower, would neceſſarily, in the courſe of the experiment, receive a ſmall portion of heat from the ſurrounding air. If we ſuppoſe that, by this cauſe, the difference of heat in the veſſels was diminiſhed the $\frac{1}{300}$ part of a degree, we ſhall have very nearly .6 for the heat which would have been imparted by pure dephlogiſticated air.

In Experiment II. the difference between the heats imparted by the dephlogiſticated and common air, was a little more than .2 of a degree. In Experiment III. it was preciſely .2. In that Experiment the heat of the water, in the tinned veſſels, previouſly to the immerſion of the cylinders, was 59.5. The cylinders were raiſed to 200, which was 140.5 degrees higher than the temperature of the water. The water was raiſed by the cylinder R, containing atmoſpherical air, a little more than one degree.

Comparing the reſult of this Experiment with that of Experiment XVI. it appears that the cylinder muſt have loſt, during its paſſage through the air, nearly $\frac{1}{5}$ of the heat which it had received from the boiling water; but the heat which it received was 140. The loſs was, therefore, 28.1: ſubtracting this from 140, and adding the remainder to the heat of the water in the tinned veſſel, we have 171.9 for the true temperature of the cylinders at the moment of their immerſion.

The general temperature of the water in the veſſel L, at the end of the experiment, was 61.7 *p*. The cylinder was therefore cooled by the water 110.2; and it has been proved, that if the cylinder had contained pure dephlogiſticated air, the heat imparted by this fluid to the water would have been .6 of a degree.

The ſpecific gravity of pure air is to that of common air, by Dr. Prieſtley's Experiments, as 187 to 185. The ſpecific gravity of the latter is to that of water as 1 to 830; conſequently the ſpecific gravity of pure air is to that of water as 1 to 821. The quantity of air was 20 ounce meaſures. The water and veſſel were equal to 21.23 ounces of water. Hence the quantity of air was to that of water as 1 to 872.49. The comparative heat of water is, therefore, to that of pure air, in the compound ratio, of 110.2 to .6, and of 1 to 872.4, or as 1 to 4.749.

The comparative heat of phlogiſticated air is calculated as follows: It has been already ſhown that the heat, which would have been imparted to the water, in the IId and IIId Experiments, by air compleatly phlogiſticated, is .108.

From the Experiments of Abbe Fontana it appears, that the ſpecific gravity of phlogiſticated air is to that of common air as 377 to 385. The ſpecific gravity of the latter is to that of water as 1 to 830; therefore the ſpecific gravity of phlogiſticated air is to that of water as 1 to 847. The quantity of phlogiſticated air was 20 ounce meaſures. The water and veſſel were equal to 21.23 ounces of water. Hence the quantity of air was to that of water as 1 to 899.

The air in the cylinder R was cooled by the water, in Experiment III. 110.4; and it was proved that the water would have been heated by the air, if it had conſiſted wholly of phlogiſticated air, .108; conſequently the comparative heat of water is to that of phlogiſticated air, in the compound ratio of 110.4 to .108, and of 1 to 899, or as 110.4 to 87, nearly; that is as 1.26 to 1.

The following is the calculation of the comparative heat of fixed air: It was ſhown, that in the experiments with the apparatus deſcribed above, which may be called an aerometer, the difference of the central heats, imparted by pure and common air to oil, was to the difference of the heats imparted by theſe

fluids

fluids to water, as 3 to 2. Hence we may conclude, that ſimilar effects would be produced if the experiments were made with common and fixed air, in oil and in water; that is, the difference in oil would be one third greater than in water; but the mean reſult of my experiments, with fixed and common air in oil, gave a difference of .05 in favour of common air. The difference in water would therefore be .034. The ſpecific gravity of fixed air was found, by the Hon. Mr. Cavendiſh, to be to that of common air as 1.57 to 1. Hence its ſpecific gravity is to that of water as 1 to 553. The quantity of fixed air was 20 ounce meaſures; the water and veſſel were equal to 21.23 ounces of water; the quantity of water was, therefore, to that of fixed air, as 587 to 1. From the foregoing experiments with the acrometer, it appears that the heat imparted, by common air, to an equal bulk of water, is .23; and we have ſeen that the heat communicated by that fluid is .034 parts of a degree greater than by fixed air. Hence the heat communicated to water by fixed air is .23—.034, or .196; and the heat ſeparated from the air by the water being 110, it follows that the comparative heat of fixed air is to that of water, in the compound ratio, of .196 to 110, and of 587 to 1, or as 115 to 110.

The comparative heat of inflammable air, as deduced from the foregoing experiments, may be calculated in the following manner.

The ſpecific gravity of pure inflammable air was found, by Mr. Cavendiſh, to be to that of common air, as 1 to 12.

The inflammable air, which was employed in the experiments to determine the comparative heat of this fluid, was procured from iron filings diſſolved in the vitriolic acid. It was obtained by a rapid efferveſcence, and was freed from the vitriolic acid air, with which it is commonly contaminated, by making it to paſs through water. It was therefore nearly pure; but as the braſs cylinder, into which it was introduced, was exhauſted by an air pump, and as the air, previouſly to its introduction into the cylinder, was forced into a bladder by the ſyringe formerly deſcribed, it was unavoidably, during this proceſs, mixed with a portion of common air. Allowance being made for the error ariſing from this cauſe, the ſpecific gravity of the inflammable air uſed in the above-mentioned experiments may be ſuppoſed to be to that of common air very nearly in the proportion of 1 to 10. But the ſpecific gravity of common air is to that of water as 1 to 830. Hence the ſpecific gravity of water was to that of the inflammable air in queſtion, as 8300 to 1.

The quantity of inflammable air was 20 ounce meaſures Troy; the water and veſſel were equal to 21.23 ounces of water. If the volumes of the air and water had been equal, the quantities of matter would have been in the ratio of 1 to 8300. But the volume of the water (the veſſel being conceived to be equivalent to 1.23 ounces of this fluid) was to that of the air as 21.23 to 20. Hence the quantity of matter in the water was to that in the air as 8810 to 1.

The heat, imparted to water by inflammable air, is a little greater than that imparted by common air in ſimilar circumſtances, as I have found by repeated trials. But as the difference is very ſmall, we ſhall probably approach near to the truth, if we ſuppoſe that the heats imparted by theſe fluids are equal. The heat therefore which inflammable air communicates to water, in experiments ſimilar to thoſe recited above, will be .23.

Hence the comparative heat of the inflammable air, which was uſed in the above-mentioned trials, is to that of water in the compound ratio of 8810 to 1, and of .23 to 113.2, or as 17.9 to 1.

Admitting that the lighteſt inflammable air communicates as much heat to water, as an equal bulk of common air, in ſimilar circumſtances, its comparative heat may be calculated as follows. The ſpecific gravity of this fluid is to that of water as 1 to 9960. The quantity of the water employed in the experiment, the veſſel being reduced to its value in water, was, therefore, to that of the air, as 10567 to 1. The comparative heat of the lighteſt inflammable air is conſequently to that of water in the compound ratio of 10567 to 1, and of .23 to 113.2, or as 21.4 to 1.

In the deductions from the foregoing experiments, it is taken for granted that the capacities of the airs are permanent, in the intermediate temperatures between the freezing and boiling points of water. But it was before obſerved, that theſe fluids probably ſuffer a ſmall alteration of capacity, in conſequence of their rarefaction or condenſation by expoſure to heat or cold. It is, however, manifeſt that no change of capacity could have ariſen from this cauſe in the experiments in queſtion; becauſe the airs were prevented from expanding, by the braſs cylinders, in which they were included. It ſeems indeed evident, from the following facts and obſervations, that ſuppoſing the airs in thoſe experiments to have been permitted freely to expand, the alteration in their capacities would have been inconſiderable.

It is well known that cold is produced when air is rarified in the receiver of an air pump, and heat when it is condenſed. I have made many experiments to determine the quantities of heat and cold which are cauſed by the condenſation and rarefaction of air; but I have not as yet been enabled to aſcertain this point with a ſufficient degree of preciſion. I have found, in general, that when one atmoſphere was ſlowly introduced into the exhauſted receiver of an air pump, a very ſenſible thermometer ſuſpended in the receiver roſe nearly 6.5 degrees. If the air was ſuddenly introduced, the increaſe of temperature was not more than 5.5; and it appeared that there was a certain limit in the velocity of introduction, which produced the greateſt poſſible augmentation of temperature.

I alſo found, that the cold produced by exhauſting the receiver, was nearly 5 degrees, and that during this operation the thermometer ceaſed to fall before the air was compleatly exhauſted, being obſerved ſenſibly to aſcend when the gage was at 27.

In theſe experiments I endeavoured to obviate the effects of moiſture, by placing the receiver upon a leather which had its pores filled with tallow. Notwithſtanding this precaution, however, a ſenſible precipitation of vapour was perceived in the exhauſted receiver. But the error ariſing from that cauſe was avoided by repeating the experiments with an inverted glaſs decanter, which was fixed to the plate of the air pump, a thermometer being inſerted by means of a ſcrew into a cavity in the neck of the decanter.

When the experiments were made with this apparatus, the reſults were nearly the ſame with thoſe of the preceding trials; the cold produced by the exhauſtion of the air being 5 degrees. It moreover appeared, that the cold generated by the rarefaction of phlogiſticated air, was a little leſs, and by the rarefaction of dephlogiſticated air, a little greater than by that of common air; but the difference was inconſiderable.

From the experiments of General Roy we learn, that the bulk of common air, at the temperature of 212, is to its bulk at the Zero of Farhenheit's thermometer, as 14 to 10, nearly; that is, when it is raiſed from Zero to 212, its expanſion is nearly one half of its original bulk.

The rarefaction of dephlogiſticated air by heat is greater than that of common air. Let the bulk of the former of theſe fluids, when its temperature is raiſed 110 degrees, be conceived to be increaſed one half; and by that increaſe of bulk, let a quantity of heat be abſorbed in conſequence of an augmentation of capacity, which would raiſe the air three degrees.

In the experiments with the aerometer recited above, it appeared that the ſeparation of 110 degrees of heat from the dephlogiſticated air, raiſed the water .4 of a degree; and hence, by the ſeparation of 110 + 3, or of

113 degrees, the water would have been raiſed .4 plus $\frac{1}{100}$. That is, the ſeparation of three degrees would have raiſed the water $\frac{1}{100}$ of a degree, which is a quantity ſo inconſiderable that it could not have been diſtinguiſhed by the thermometer.

Having determined the comparative heats of atmoſpherical, pure, phlogiſticated, fixed, and inflammable air, I next endeavoured to aſcertain the comparative heat of aqueous vapour.

The comparative heat of aqueous vapour could not be determined by experiments ſimilar to thoſe recited above, becauſe the vapour of boiling water condenſes when it is reduced to a temperature below 212. I therefore had recourſe to the following method in order to diſcover the capacity of that fluid.

It is now generally believed that aqueous vapour is compoſed of pure and inflammable air, which in the proceſs of combuſtion intimately unite with each other, and at the ſame time give off a large quantity of elementary fire.

If, therefore, the whole quantity of abſolute heat, contained in pure and inflammable air, be given, and if the heat which theſe fluids evolve, when, by the proceſs of combuſtion, they are changed into aqueous vapour, be likewiſe determined, the quantity of abſolute heat in the latter will be known. Hence, in order to diſcover the quantity of heat in aqueous vapour, it was neceſſary to determine the heat which is extricated from pure and inflammable air when they are burned together in cloſe veſſels. With a view to determine the heat produced by the combuſtion of theſe fluids, it was propoſed to me by Dr. Prieſtley that they ſhould be confined in a tinned or braſs veſſel ſurrounded with water, and ſhould be fired by the electric ſpark, a thermometer being introduced into the water to indicate the heat that it received: in conſequence of which I contrived the following apparatus for the purpoſe of making this experiment. (See plate III. fig. 3.) G K repreſents a braſs veſſel, at the lower extremity of which is fixed a ſtop cock, and at the upper extremity the apparatus commonly uſed for firing inflammable and dephlogiſticated air by the electric ſpark. This apparatus conſiſts of a crooked braſs wire O P, inſerted in a ſmall glaſs tube *r*, to which it is cemented by plaſter of Paris. The glaſs tube is, in like manner, cemented to a braſs pipe through which it paſſes, the latter being fixed by a ſcrew to the braſs veſſel at the point L. The immediate communication between the wire and veſſel being thus interrupted by means of the glaſs tube, the electric ſpark, which enters at the upper extremity of the wire, ſets fire to the air in its paſſage from the lower extremity Q to the ſide of the veſſel.

The airs having been mixed in the proper proportions in a bladder, are introduced into the braſs veſſel previouſly exhauſted by an air pump. The braſs veſſel is then introduced into the tinned cylindrical veſſel A D, (ſee fig. 4) to the bottom of which it is ſcrewed by means of a male ſcrew formed on the extremity of the pipe of the ſtop cock, which, as has been already mentioned, is fixed to the lower end of the braſs veſſel. As this cock neceſſarily projects to a little diſtance from the pipe, through which the air paſſes, it is manifeſt that a ſmall portion of vacant ſpace will be interpoſed between the pipe and the lower extremity of the tinned veſſel which is formed to receive it. For the purpoſe of filling this cavity, a piece of cork, equal to it in ſize, is divided longitudinally through the centre, and its ſides are applied to the ſtop cock, being excavated in ſuch a manner as to be cloſely adapted to every part of it. This is repreſented by N O, the lower extremity of the figure G K, ſee fig. 3.

Finally, a quantity of water, ſufficient to cover the braſs veſſel, is introduced into A D, (ſee fig. 4) and the temperature is indicated by a thermometer, which has each degree of Farhenheit's ſcale divided into ten equal parts. The tinned veſſel is covered with thick flannel for the purpoſe of counteracting the refrigerating power of the ſurrounding air.

By means of this apparatus I made the following experiment:

Air in the room — 48.

A mixture of dephlogiſticated and inflammable air, the volume of the latter being to that of the former as 2 to 1, was introduced into the braſs veſſel, which was ſcrewed to the bottom of the tinned veſſel, and ſurrounded with 16 ounces Troy, 2 drams, and 2 ſcruples of water The water being ſuffered to remain in the veſſel till the heat became very nearly ſtationary, its temperature, as indicated by a thermometer which had each degree of Farhenheit's ſcale, divided into ten equal parts, was found to be 46.1 plus.

The airs were now fired by the electric ſpark, and the temperature

In 1 minute was	—	47.3
2	— —	47.9
3	— —	48.3
4	— —	48.4
5	— —	48.5
6	— —	48.6
7	— —	48.6
8	— —	48.6 plus.
9	— —	48.6 plus.

The braſs veſſel being now removed, and the water being agitated, for a little time, that the heat might be equally diffuſed over it, the general temperature was 48.6 plus. When this experiment was made the barometer ſtood at 30.6.

The purity of the dephlogiſticated air, as meaſured by Dr. Prieſtley's Teſt, was .2. The inflammable air was obtained from iron filings diſſolved in the vitriolic acid.

It is proper to obſerve, that as the braſs veſſel could not be compleatly exhauſted by the air pump, a ſmall portion of common air remained in it when the mixture of dephlogiſticated and inflammable air was introduced; a little common air was alſo unavoidably mixed with theſe fluids, when they were forced by means of a ſyringe into the bladder, previouſly to their introduction into the braſs veſſel. Though the inflammable air was obtained by a rapid efferveſcence, and was paſſed ſeveral times through water, for the purpoſe of purifying it, yet it could not be entirely freed from the vitriolic acid air, and other impurities with which it is commonly contaminated.

For theſe reaſons, after the experiment was finiſhed, a quantity of air remained in the braſs veſſel, which did not ſuffer any change by the deflagration. This quantity was eſtimated in the following manner: the braſs veſſel, at the concluſion of the experiment, being unſcrewed from the tinned veſſel, was inſtantly plunged into water, and the cock, which was fixed to its lower extremity, being opened, the water was ſuffered to ruſh into it. Having remained in this ſituation until no more water would enter, the cock was ſhut, and the veſſel was weighed; after which the ſame veſſel being compleatly filled with water,

ter, was again weighed: whence it appeared that 5 ounce meaſures Troy, and 3 dram meaſures of air, remained in the veſſel at the concluſion of the experiment.

The original heat of the water in this experiment was 46.1 plus. At the end of nine minutes, when the braſs veſſel was removed, and the water was reduced by agitation to a uniform temperature, the thermometer ſtood at 48.6 plus. The heat imparted by the deflagration of the airs was therefore 2.5 degrees.

This experiment was frequently repeated, and the mean reſult gave 2.4 for the heat communicated to the water. Hence the number of degrees of ſenſible heat, which would have been communicated by the deflagration of the airs employed in theſe experiments, to an equal quantity of pure and inflammable air, mixed in the ſame proportions, may be determined as follows.

The weight of the braſs veſſel in which the airs were fired, was 23 ounces Troy, 1 dram, and 2 ſcruples, or 11140 grains. The capacity of braſs is to that of water as 1 to 8.9. That is, 8.9 ounces of braſs, or 4272 grains, are equal in capacity to 1 ounce of water; and, conſequently, 23 ounces, 1 dram, and 2 ſcruples, or 11140 grains, are equal in capacity to 2.607 ounces of water.

The capacity of the tinned veſſel in which the water and braſs apparatus were contained, was to that of 34 ounces Troy of water, as 1 to 21.4. In other words, the quantity of abſolute heat contained in the veſſel was equal to that contained in the $\frac{1}{21.4}$ part of 34 ounces of water, or equal to that contained in 1.58 ounces of water. The quantity of water employed in the experiment was 16 ounces Troy, 2 drams, and 2 ſcruples, or 16.33 ounces very nearly. The braſs apparatus has been ſhown to be equal to 2.607 ounces of water, and the tinned veſſel to 1.58 ounces. Hence the water, cylinder, and veſſel were equal in capacity to 20.517 ounces of water.

It was the purer part of the inflammable air alone that was capable of uniting with the dephlogiſticated air in theſe experiments.

The ſpecific gravity of pure inflammable air is to that of common air as 1 to 12, and to that of dephlogiſticated air nearly as 1 to

12.1. If, therefore, the volumes of the two airs, uſed in the experiments in queſtion, had been equal, their weights would have been as 1 to 12.1. But as the volume of the inflammable air was twice as great as that of the dephlogiſticated air, the former was to the latter in weight as 1 to $\frac{12.1}{2}$, or as 1 to 6.05. Hence the ſpecific gravity of the compound, conſiſting of theſe two ſpecies's of air mixed in the above proportions, was to that of dephlogiſticated air, as $\frac{1}{2.574}$ to 1, and to that of inflammable air, as $\frac{1}{2.574}$ to $\frac{1}{12.1}$, or as 4.7 to 1; and the ſpecific gravity of inflammable air being to that of water, as 1 to 9960, it follows that the ſpecific gravity of water was to that of the compound as 2119 to 1.

The comparative heat of pure dephlogiſticated air is to that of water, as 4.75 to 1, nearly; and the comparative heat of pure inflammable air is to that of water, as 21.4 to 1. Hence the comparative heat of the compound was to that of water, as 7.11 to 1.

It was ſhown above that the quantity of air which remained in the braſs veſſel, at the concluſion of the experiment, was 5 ounce meaſures Troy, and 3 dram meaſures. It is manifeſt that this air, which conſiſted chiefly of phlogiſticated, mixed with a ſmall portion of common air, did not ſuffer any change in its capacity during the deflagration.

This appears from the experiments of Mr. Cavendiſh, who has proved that pure dephlogiſticated and inflammable air are almoſt wholly converted into water, when they are fired by the electric ſpark in cloſe veſſels; and that the quantity of air which remains in the veſſel, at the concluſion of the experiment, is in proportion to the heterogeneous aërial fluids with which thoſe airs are mixed previouſly to their combuſtion.

The entire quantity of air which filled the braſs veſſel was 16 ounce meaſures Troy, and 3 dram meaſures, very nearly; and the quantity which remained unaltered being 5 ounce meaſures, and 3 dram meaſures, it follows that the volume of the airs, which contracted a chemical union in thoſe experiments, and which were converted into water, was 11 ounce meaſures. The quantity of water uſed in the experiments was 16.33 ounces; and it was before ſhown that the braſs and tinned veſſels were equal in capacity to 4.187 ounces of water; conſequently the relative magnitudes of the air and water were as 11 to 16.33 + 4.187, or as 11 to 20.517.

It has been already proved that the ſpecific gravity of the compound of the airs employed in the experiment was to that of water as 1 to 2119. Hence the quantity of matter in 20.517 ounces of water was to that in 11 ounce meaſures of the compound, as 3952.3 to 1. We have ſeen that the water was raiſed, by the deflagration of the airs, 2.4 degrees. If the capacities of the airs and water had been equal, it is manifeſt that the ſame heat which raiſed 3952 parts of water, 2.4, would have raiſed one part of air 9484.8 degrees. But the capacity of the airs was to that of water as 7.11 to 1. The airs would therefore have been raiſed by the ſame heat only the $\frac{1}{7}$ part of 9484.8, or 1333 degrees. Hence the whole quantity of heat which the airs contain at a given temperature, as expreſſed in degrees of the thermometer, may be determined as follows.

It was ſhown, that the airs previouſly to their combuſtion contained a quantity of heat ſufficient to raiſe them 1333 degrees. If we ſuppoſe, that when they were fired by the electric ſpark they gave off the whole of the heat which they contained; it will follow that the point of total privation, or the degree of cold to which they muſt be reduced in order to deprive them wholly of their heat, is 1333 degrees below the common temperature of the atmoſphere. But a part of the heat that exiſted in theſe fluids was retained by the water, which was the product of their union; the heat that was extricated from the airs being the difference between the entire quantity of heat which they contained, and that contained in an equal weight of water at the ſame temperature. We have before ſeen that the whole quantity of abſolute heat, in the compound of pure and inflammable air, was to that contained in an equal weight of water at the ſame temperature as 7.11 to 1; and hence the difference between the abſolute heat of the airs and of the water was 7.11—1, or 6.11.

It has been proved that this difference was ſuch as would have heated the compound of pure and inflammable air 1333 degrees. In order, therefore, to diſcover how much the temperature of the airs would have been raiſed by the heat which was retained by the water;

ter; ſay if 6.11 give 1333 degrees, what will 1 give? Whence it appears, that the heat retained by the water would have heated the airs 218 degrees; adding this to 1333, we have 1551 degrees for the whole quantity of heat contained in the airs, as expreſſed in degrees of the thermometer, and eſtimated from the point of total privation *.

The reader will obſerve, that the foregoing calculation is deduced from the comparative heats of air, and of water, and not from thoſe of air and aqueous vapour. For though, in the experiments recited above, the pure and inflammable air were at firſt converted into aqueous vapour; yet as the veſſel was ſurrounded with water at a low temperature, and as a conſiderable quantity of air remained in it after the deflagration,

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* It is a tribute of juſtice, which is due to Dr. Irvine, to acknowledge, that the mode of reaſoning, which has been employed in the above enquiry, to determine the whole quantity of heat contained in air, is ſimilar to that which he formerly employed to calculate the loweſt degree of heat in bodies from the relative capacities of ice and water, and the quantity of heat given off by the latter when it is congealed. The ingenious Theorem, by which this curious queſtion was ſolved, the reader will ſee explained in Mr. Nicholſon's Treatiſe on Natural Philoſophy.

the vapour, in the courſe of the experiment, muſt neceſſarily have been condenſed.

It is evident that, in this experiment, an imperfect vacuum was formed by the converſion of the pure and inflammable air into water; and ſome Philoſophers have ſuppoſed that a vacuum may contain heat.

I endeavoured to determine the queſtion reſpecting the capacity of a vacuum for heat by the following experiment: The cylindrical veſſels of the aerometer, containing atmoſpherical air, were expoſed for a conſiderable time, in the bath, to the action of boiling water, in order that the ſtop cocks, by having the heat uniformly diffuſed over them, might become air tight. The air was then exhauſted from one of the cylinders by means of a crooked pipe, which was ſcrewed upon it, and which communicated with an air pump; after which the cylinders having been expoſed to heat in the bath for ſeveral minutes, were ſuddenly immerſed in the tinned veſſels, containing equal quantities of cold water. From this experiment, which was ſeveral times repeated, it clearly appeared that a vacuum has a leſs capacity for heat than atmoſpherical air; and that, if it does contain heat, the quantity is ſo minute that it could not be appreciated by my thermometers. Admitting, however, that a ſmall quantity of heat may exiſt in the vacuum, which is formed by the deflagration of pure and inflammable air, in cloſe veſſels, it will follow, that the natural Zero is a little lower than that which has been derived from the foregoing calculation. And in that caſe the natural Zero, as determined by experiments with ſubſtances which do not undergo a conſiderable alteration of bulk, when they ſuffer a change of form, would be farther removed from the common temperature of the atmoſphere, than the Zero which is deduced from experiments with pure and inflammable air. It is manifeſt, however, that the utmoſt care and accuracy would be required to determine this point with ſufficient preciſion. In the mean time I think we may conclude with certainty, that pure and inflammable air, at the temperature of 50, contain nearly 1550 degrees of heat.

It was before ſhown that all bodies, which have a common temperature, muſt have the ſame number of degrees of ſenſible heat, as meaſured by an equidifferential thermometer, and eſtimated from the point of total privation. If, therefore, the entire quantity of ſenſible heat contained in air, at the common temperature of the atmoſphere, be 1550 degrees, it will follow that the number of degrees of heat, in all bodies at the ſame temperature, muſt likewiſe be 1550; and conſequently this will be the point to which, if bodies were to be refrigerated, they would become abſolutely cold.

Hence it is manifeſt, that if, by the foregoing experiments upon air, the true Zero, or point of abſolute cold, has been juſtly determined, it ſhould correſpond with the Zero which is derived from ſimilar experiments upon all other bodies that produce ſenſible heat or cold, in conſequence of a change of form. Meſſrs. Lavoſier and De la Place have endeavoured to ſhow that the Zero, or point of total privation, as determined by the changes of capacity, and the quantities of heat evolved from bodies, in various inſtances, is not uniform. But they admit that the apparent deviations from this law were not greater than ſuch as might have ariſen from unavoidable errors in their experiments. I have made many experiments with a view to determine this queſtion; and I have uniformly found, that when bodies produced heat, in conſequence of a change of form, their capacities were diminiſhed; and that, on the contrary, when they produced cold, their capacities were increaſed. But I have not yet been enabled to bring my experiments to ſuch a degree of accuracy as to determine, with certainty, whether the relation which the increaſe and diminution of capacity bears to the quantities of heat and cold produced be ſuch as to give a uniform reſult for the point of total privation. The experiments were, however, by no means inconſiſtent with that law, and did not deviate from it more than might naturally be expected from the inaccuracy to which ſuch experiments are liable.

Having aſcertained the whole quantity of heat in pure and inflammable air, we may hence determine the capacity of aqueous vapour. It has been proved that pure air, at the common temperature of the atmoſphere, contains 1550 degrees of heat; and it was ſhown that all other bodies muſt contain an equal number of degrees when at the ſame temperature. Water, therefore, at the common temperature of the atmoſphere, will likewiſe have 1550 degrees of heat; and at the temperature of 212 it will have nearly 1650 degrees of heat. But the heat abſorbed by aqueous vapour, as appears from the experiments of Mr. Watt, is 914 degrees. From which it follows, that the capacity of water is to that of aqueous vapour as 1650 to 1650 + 914, or as 1 to 1.55. It was before proved that the capacity of water is to that of pure air as 1 to 4.749. The capacity of aqueous vapour is therefore to that of pure air as 1.57 to 4.749, or as 1 to 3, nearly.

Thus it appears, that dephlogiſticated air contains a greater quantity of abſolute heat than fixed or phlogiſticated air, or aqueous vapour.

Hence the quantity of heat, in a mixture of pure and phlogiſticated air, will be encreaſed by augmenting the proportion of the former, and diminiſhed by augmenting that of the latter. Agreeably to this we find, that the comparative heat of air perfectly pure, is to that of atmoſpherical, which conſiſts of $\frac{1}{4}$ pure, and $\frac{3}{4}$ phlogiſticated air, as 2.2 to 1.

The reader will obſerve, that the quantities of abſolute heat in equal bulks of reſpirable air, having different degrees of purity, are not accurately proportional to the quantities of pure air which they contain; for the quantity of pure air, in a given portion of the pureſt dephlogiſticated air, is four times as great as that contained in an equal bulk of atmoſpherical; and the comparative heat of the former is to that of the latter only as 2.2 to 1. The reaſon is obvious: phlogiſticated air, which is one of the conſtituent parts of atmoſpherical, contains a conſiderable quantity of abſolute heat.

If, however, from the abſolute heat of any ſpecies of reſpirable air, we ſubtract the heat contained in that portion of it, which is not altered by the action of the lungs in reſpiration, the quantity of heat contained in the remainder will be nearly proportional to its power in ſupporting animal life. Thus, if from the heat of a given quantity of atmoſpherical air, we deduct the heat of the phlogiſticated air which forms one of its conſtituent parts, and which is not altered by reſpiration; the quantity of heat in the remainder will be to that contained in a quantity of pure air equal in bulk to the atmoſpherical, as 1 to 4 nearly. And Dr. Prieſtley, whoſe diſcoveries on this ſubject have been deſervedly much admired, has proved, that the power of the latter, in ſupporting animal life, is nearly five times as great as that of the former.

It is proper to remark, that though the powers of different ſpecies's of reſpirable air, in contributing to the ſupport of animal life, are nearly proportional to the quantities of pure air which they contain, yet they are not accurately in that proportion; for the quantities of pure air, contained in the pureſt dephlogiſticated and in atmoſpherical-air, are as 4 to 1; but the power of the firſt, in ſupporting animal life, is to that of the laſt as 5 to 1. The reaſon of this difference ſeems to be, that a ſmall portion of the pure air, contained in the atmoſpherical, is protected from the action of the lungs by the phlogiſticated air with which it is mixed, as Mr. Kirwan has ſuggeſted.

There is, therefore, upon the whole, ſufficient evidence for concluding that the purer part of atmoſpherical air has its capacity for heat diminiſhed by the change which it undergoes in the lungs of animals; and that, if from the abſolute heat of any ſpecies of reſpirable air not perfectly pure, we ſubtract the heat of thoſe ingredients which are not altered by reſpiration, the quantity of heat contained in the remainder will be nearly proportional to its power in ſupporting animal life.

PROPOSITION II.

The blood which paſſes from the lungs to the heart by the pulmonary vein, contains more abſolute heat than that which paſſes from the heart to the lungs by the pulmonary artery.

As the former is the blood which is propelled through the arteries in the aortic ſyſtem, and the latter is that which in the ſame ſyſtem is returned by the veins, I ſhall call the firſt arterial, and the laſt venous blood.

The following experiments were made to determine the comparative heats of venous and arterial blood.

EXPERIMENT I.

Air in the room — 68

Half a pound of water, Avoirdupoiſe weight, at 53, was mixed with half a pound and 400 grains of arterial blood, at 102.

The mixture at the end of
1 minute was 78,
2 — 77¾, nearly,
3 — 77½ when it coagulated,
4 — 77½.

EXPERIMENT II.

Half a pound of water, Avoirdupoiſe weight, at 53½, was mixed with nine ounces and a half and 14 grains of venous blood, at 99½.

The mixture at the end of
1 minute was 76,
3½ — 76 when it coagulated,
8 — 76,
9 — 75¾.

In making theſe experiments it was neceſſary to uſe as much expedition as poſſible, that the heat of the mixture might be determined previouſly to the coagulation; and, therefore, the water was firſt accurately weighed. Half a pint of blood was taken from the carotid artery of a ſheep for the firſt experiment, and from the jugular vein for the ſecond: the heat of the mixture was then aſcertained by the thermometer, and the weight of the blood was determined at the concluſion of the experiment.

It may be proper to obſerve, that when the venous and arterial blood were firſt diſcharged from the animal, the former was much more fluid than the latter; and we have ſeen that when they were mixed with equal quantities of water, the venous blood was ſomewhat later in coagulating than the arterial.

To determine the heat of arterial blood, from the former of the above experiments, we may obſerve, that as, in this experiment, the blood was poured upon the water, a ſmall portion of heat was loſt in its paſſage through the air. I have found, by a ſubſequent trial, that the quantity of heat which was thus loſt was very nearly one degree. If this heat had been added to the mixture, it would have raiſed it nearly half a degree; and as the mixture, previouſly to its coagulation, cooled at the rate of one-fourth of a degree in a minute, we may add, at leaſt, half a degree for the heat loſt in the firſt minute, which gives 78½ for the temperature of the mixture.

This experiment was made in a pint pewter veſſel, the capacity of which for receiving heat was to that of the water nearly as 1 to 16. The quantity of water was eight ounces; the heat received by the veſſel was conſequently equal to that which would have been received by the one-ſixteenth part of eight ounces, or by half an ounce of water. It follows, that the effect of the veſſel and the water together was equal to that which would have been produced by eight ounces and an half of water.

The temperature of the mixture was 78½; ſubtracting this from 102, we have 23½ for the heat ſeparated from the blood. The water and the veſſel were raiſed from 53 to 78½, or 25½. The quantity of blood was eight ounces and 400 grains Avoirdupoiſe, or 3899 grains; and, therefore, the heat of the arterial blood is to that of water, in the compound ratio of 8½ ounces to 8 ounces and 400 grains, and of 25½ to 23½, or as 103 to 100; conſequently the heat of water is to that of arterial blood as 100 to 103, or nearly as 97.08 to 100.

In the ſecond experiment, adding half a degree for the heat loſt in the firſt minute, we have 76½ for the temperature of the mixture. The blood was cooled from 99⅓ to 76½, or nearly 22.83. The water and veſſel were raiſed from 53½ to 76½, or 23 degrees. The quantity of venous blood was 9½ ounces and 14 grains Avoirdupoiſe, or 4168 grains. The water and veſſel were together equal to 8½ ounces of water; therefore the heat of venous blood is to that of water, in the compound ratio of 8½ ounces to 9½ ounces and 14 grains, and of 22.83 to 23, or as 100 to 112.

Putting A for arterial blood, V for venous, and W for water, the ratio of the heat of venous to that of arterial blood, is determined in the following manner:

V.	W.	A.
97.08.	100.	112.

Therefore V : A : : 97.08 to 112, or nearly as 10 to 11½

To determine whether the ſame change is produced in the blood of other animals, by the proceſs of reſpiration, ſimilar experiments were made with the venous and arterial blood of a dog. And the inaccuracy which might be ſuppoſed to have ariſen in the former experiments, from the tendency of the blood to coagulate, was avoided by mixing the water and blood in ſuch quantities that the coagulation ſhould not take place for many minutes. When nearly 2 ounces of venous blood was received from the jugular vein of a dog, and ſpeedily mixed with 13 ounces of water, no tendency to coagulation was obſerved during the firſt three minutes. At the end of the 4th minute, the fluidity of the mixture ſeemed to be a little diminiſhed; and at the end of the 6th minute it was compleatly coagulated.

When 1 ounce, 7 drams, and 2 ſcruples of arterial blood was taken from the carotid artery of a dog, and mixed with 13 ounces Troy of water, having the ſame temperature with that uſed in the foregoing experiment, a tendency to coagulation was perceived at the end of 3½ minutes; and at the concluſion of the 5th minute, the coagulation was compleated. By varying theſe experiments, I found that when one part of blood was mixed with nearly 12 parts of water, no coagulation took place for ſeveral hours; and thus the inaccuracy which might be ſuppoſed to ariſe from a tendency to coagulation, was obviated. From the reſults of a variety of trials, made with theſe precautions, it appeared that the comparative heat of the arterial blood of a dog was to that of the venous, as 114 to 100. In the experiments with the blood of a ſheep, the comparative heats were as 115 to 100. Theſe reſults ſo nearly coincide, that the difference may be ſuppoſed to have ariſen from unavoidable errors in the experiments. It is not improbable, however, that in ſome animals, the exceſs of the comparative heat of the arterial above that of the venous blood, may be greater than in others.

From the foregoing experiments we may conclude, in general, that the blood which paſſes from the lungs to the heart, by the pulmonary vein, contains more abſolute heat than that which paſſes from the heart to the lungs by the pulmonary artery.

PROPOSITION III.

The comparative quantities of heat in bodies, ſuppoſed to contain phlogiſton, are encreaſed by the changes which they undergo in the proceſſes of calcination and combuſtion.

By ſome Philoſophers in the preſent age, the exiſtence of the principle, which the Chemiſts have termed phlogiſton, has been called in queſtion.

It does not, however, appear that any evidence has hitherto been adduced which will warrant us in rejecting this principle. We know that a particular ſpecies of combuſtion is produced by the union of inflammable with the purer part of common air. We alſo know that inflammable air can be obtained from the greater part of combuſtible bodies, by expoſing them to heat in cloſe veſſels. Hence this fluid or its baſis exiſts in moſt of thoſe bodies; and as they are not found to contain any other principle eſſential to their inflammation, it follows that inflammable air or its baſis may juſtly be conſidered as the ſame thing with the phlogiſton of the Chemiſts *. But it may be doubted whether the Chemiſts have not been deceived, in ſuppoſing the extrication of this principle

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* See Mr. Kirwan's Paper on this ſubject. Philoſophical Tranſactions, Royal Society, London, vol. 72, p. 195. See alſo the treatiſe which has lately been publiſhed, by the ſame Philoſopher, in defence of the doctrine of phlogiſton.

neceſſary in all caſes to that proceſs, as well as to the proceſs of calcination.

If there is any other ſubſtance in nature, beſides the inflammable principle, which is capable of combining with pure air; and if, in conſequence of that combination, the air ſhould ſuddenly ſuffer a great diminution in its capacity for heat, the phenomena of flame and of combuſtion would be produced.

Mr. Lavoſier, who is the principal opponent of the doctrine of phlogiſton, ſuppoſes that metals are ſimple ſubſtances, which in certain degrees of heat are capable of contracting an union with pure air, and that by this union they are converted into calces. That when the calces are again expoſed to heat in contact with charcoal, the latter combines with the pure air, changing it into aërial acid; and thus the air being ſeparated by the ſuperior attraction of the charcoal, the metals are revived. Sulphur and phoſphorus, are, in like manner, ſuppoſed to be ſimple ſubſtances, which during their combuſtion are changed into acids by combining with pure air.

This theory appears neceſſarily to lead to the following conſequences. If the inflammable air, which is produced by the ſolution of metals in acids, by that of ſulphur in alkalis, and by the expoſure of charcoal to heat, be not derived from the metals, the ſulphur and the charcoal, but from the decompoſition of water, as Mr. Lavoſier ſuppoſes, the quantity of inflammable air produced in theſe caſes, ſhould not be greater or leſs, than the quantity contained in the portion of water that is decompoſed. It is now generally believed, that the quantity of inflammable air contained in water, is nearly $\frac{1}{6}$ of the whole, the remaining $\frac{5}{6}$ conſiſting of pure air. During the ſolution of metals in acids, Mr. Lavoſier conceives that the portion of the water, which conſiſts of pure air, is united to the metallic ſalt, the portion which conſiſts of inflammable air being at the ſame time ſuffered to eſcape. When the metallic ſalt is decompoſed by a cauſtic alkali, the air that was formerly united to the ſalt adheres to the calx, and falls with it to the bottom of the veſſel. The weight of the precipitate is conſequently greater than the original weight of the metal.

Hence the quantity of water that is decompoſed, admitting the truth of Mr. Lavoſier's ſier's theory, may be determined, for it will be $\frac{1}{5}$ greater than the exceſs of the weight of the calx above the original weight of the metal *. If, therefore, that exceſs be divided into five equal parts, and if the weight of the inflammable air be equal to one of thoſe parts, we may conclude, with much appearance of probability, that it was wholly derived from the water. If it be greater, we may thence infer, with certainty, that a part of it muſt have been extricated from the metal during its ſolution; and if leſs, it will follow that the whole of the air united to the calx could not have been derived from the decompoſition of water.

It appears, indeed, from Mr. Kirwan's late excellent work on phlogiſton, that the hypotheſis reſpecting the decompoſition of water, is not ſupported by any deciſive experiments;

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* Dr. Prieſtley's Experiments prove, that in ſome inſtances water is obtained by expoſing the calces of metals to heat in contact with inflammable air. In thoſe caſes the quantity of water that was decompoſed during the calcination of the metals, according to Mr. Lavoſier's Hypotheſis, may be found, for it will be equal to the quantity that is reproduced.

periments; and, conſequently, that the opinion of thoſe who reject the antient doctrine of phlogiſton reſts on a very precarious foundation.

As the queſtion, however, reſpecting the exiſtence of the inflammable principle in metals, in charcoal, in ſulphur, and in phoſphorus, does not yet ſeem to have been determined with ſufficient certainty, I ſhall not enter into it more particularly at preſent; but ſhall endeavour to ſhow that, whatever be the nature of the change which bodies, ſuppoſed to contain phlogiſton, undergo in the proceſſes of calcination and combuſtion, the quantities of their abſolute heat are encreaſed. This will appear from the following experiments.

EXPERIMENT I.

Air in the room being —	49.
16 ounces of water were introduced into a tinned veſſel, and were raiſed to —	127.
With this water 18 ounces of waſhed diaphoretic antimony were mixed at —	49.1.

The veſſel containing the mixture was placed in the ſteam bath formerly deſcribed. It reſted on a thin piece of cork which was applied to the bottom of the bath, and its mouth was cloſed with a cork to prevent the acceſs of the external air. The bath was raiſed to the fixed temperature of 102.

The mixture being agitated in ſuch a manner as to bring its ſeveral parts nearly to an equal heat, in two minutes its temperature (as meaſured by a cylindrical thermometer which reached from the ſurface to the bottom) was — 111.75,
6 minutes — 111.75,
8 — 111.75, nearly.

By ſubſequent trials it was found that the agitation given to the mixture diminiſhed its temperature nearly .33 of a degree. Adding therefore .33 to 111.75, we have 112.08 for the true temperature of the mixture.

In this experiment the cold antimony was introduced into the veſſel containing the warm water. The heat imparted by the veſſel was equal to that which would have been imparted by .85 of an ounce of water.

The water was cooled — 14.92.
and the antimony heated — 62.58.

Since, therefore, the antimony cooled 16 ounces of water, and the veſſel which was equal to .85 of an ounce of water, 14.92 degrees, it follows that it would have cooled 16.85 ounces of water 14.92 degrees. Hence the comparative heat of water is to that of diaphoretic antimony, in the compound ratio of 14.92 to 62.58, and of 16.85 to 18, or as 4.4 to 1, nearly.

As the calx uſed in this experiment was united to a conſiderable quantity of air, I next endeavoured to expel the air from it, as compleatly as poſſible, by moiſtening it with nitrous acid, and expoſing it to a red heat, agreeably to the method recommended by Mr. Scheele. Its comparative heat, after this proceſs, was found to be to that of water as 1 to 6; and the ratio of the comparative heat of regulus of antimony was, to that of the ſame fluid, as 1 to 15.5.

By ſimilar experiments, I endeavoured to determine the comparative heats of moſt of the metals and their calces, the latter being examined both when combined with air, and when in a great meaſure freed from it by means of nitrous acid, and expoſure to heat.

The reſults are comprehended in the following table:

Waſhed diaphoretic antimony	.22727
Waſhed diaphoretic antimony, having the air extricated by nitrous acid and fire, -	.16666
Regulus of antimony -	.06451
Yellow calx of lead having the air extricated * -	.06802
Lead - -	.03520
White calx of tin -	.10869
White calx of tin, having the air extricated by nitrous acid and fire -	.099009
Tin - -	.070422
Ruſt of iron - -	.25000
Ruſt of iron, having the air extricated by nitrous acid and fire, - -	.16666
Iron - -	.12696

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* This calx was obtained from red lead by means of nitrous acid and fire. The comparative heats of the red lead and yellow calx were found to be nearly the ſame.

Calx of copper precipitated from blue vitriol by an alkali, and having the air extricated by nitrous acid and fire, -	.22727
Copper - -	.11111
Braſs - -	.11235
Calx of zinc precipitated from white vitriol by an alkali, and having the air extricated by nitrous acid and fire, -	.13698
Regulus of zinc -	.09433
Water - -	1.00000

By ſimilar experiments it may be proved that ſulphur contains leſs abſolute heat than the vitriolic acid, and oil leſs than water or air *.

From the following trials, which were ſeveral times repeated with the utmoſt care, it appears that alcohol alſo contains leſs abſolute heat than water.

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* This appears from Mr. Kirwan's Experiments, the reſults of which have been communicated to the public, in Mr. Magellan's Treatiſe on Elementary Fire. According to theſe experiments the comparative heat of

water being — —	1.000
That of ſulphur is —	0.183
Vitriolic acid, of a brown colour, —	0.429
Linſeed oil — —	0.528

Ex-

EXPERIMENT I.

Air in the room — 61.5.
Nine ounces of powdered chalk at 61,
mixed with 10¼ ounces of ſpirit
of wine at — 139.

The mixture being agitated 20 times with the thin blade of a knife, and the temperature being examined by a cylindrical thermometer which reached from the ſurface to the bottom,

In 1 minute it was 120.75
2 — 120.5 nearly
3 — 119.75
4 — 119.

I have found that the parts of the mixture are moſt ſpeedily brought to a common temperature when they are agitated by a thin flat ſubſtance. The blade of a knife is well calculated for this purpoſe.

From ſubſequent trials it appeared, that the heat loſt by the agitation in the courſe of the firſt minute was one degree. Adding this to 120.75, we have 121.75 for the true temperature of the mixture.

The ſpirit was therefore cooled 17.25,
and the chalk heated - 60.75.

In the experiment the cold chalk was poured into the veſſel containing the warm ſpirit of wine; and the capacity of the veſſel being equal to that of .7 of an ounce of ſpirit, the ſpirit and veſſel were together equal to 10.95 ounces. The quantity of chalk was nine ounces. Hence the comparative heat of chalk is to that of ſpirit of wine, in the compound ratio of 17.25 to 60.75, and of 10.95 to 9, or as 1 to 2.89. The ſpecific gravity of the ſpirit uſed in this experiment was .857

The following experiment was made with ſpirit, the ſpecific gravity of which was .784.

EXPERIMENT II.

Air in the room —	60.
Nine ounces of powdered chalk at	62.8,
mixed with 10.25 ounces of alcohol at —	139.

The mixture being agitated 20 times with the thin blade of a knife, its temperature, as meaſured by a cylindrical thermometer, was

In 1 minute —	118.75
2 — —	118.5
3 — —	118
4 — —	117.

Allowing one degree for the heat loſt by agitation in the courſe of the firſt minute, we have 119.75 for the true temperature.

The alcohol was therefore
cooled — 19.25
and the chalk heated 56 degrees.

The cold chalk was introduced into the veſſel containing the warm alcohol; and the capacity of the veſſel being nearly equal to that of .8 of an ounce of alcohol, the ſpirit and veſſel were together equal to 11.15 ounces. The quantity of chalk was nine ounces.

The comparative heat of chalk is therefore to that of alcohol in the compound ratio of 19.25 to 56, and of 11.15 to 9, or as 1 to 2.34.

EXPERIMENT III.

Air in the room — 61.
Nine ounces of powdered chalk at 62.2,
mixed with 10.5 ounces of water at — 139.

The mixture being agitated as before 20 times, and its temperature being meaſured by the ſame thermometer that was uſed in the former experiments,

In 1 minute it was		-	124.75
2	—	—	124.5 plus,
3	—	—	123.75
4	—	—	122.75.

The capacity of the veſſel was equal to that of .65 of an ounce of water, and the heat loſt in the firſt minute was very nearly 1.2 degrees. Hence by this experiment the comparative heat of chalk is to that of water as 1 to 3.9.

The comparative heat of water is therefore to that of alcohol as 3.9 to 2.34, or as 1.66 to 1.

This experiment was repeated as follows:

Air in the room — 52.

Atmoſpherical air being contained in both tubes of the aerometer, 16½ ounces of ſpirit of wine (ſpec. grav. .826) was introduced into the tinned veſſel L, and 20 ounces of water into R.

The temperatures of the water and ſpirit being very nearly equal, the aerometer was heated in the water bath, and immerſed in the tinned veſſels, where it was ſuffered to remain during 16 minutes; after which being removed, and the water and ſpirit being agitated for a ſhort time, the former was raiſed two degrees, the latter four. By this experiment the comparative heat of water is to that of the ſpirit as 1.5 to 1.

From the foregoing experiments it appears, in general, that metals contain leſs abſolute heat than their calces, ſulphur leſs than the vitriolic acid, and oil and alcohol leſs than air or water. Moſt of the metals, which were the ſubject of theſe experiments, are fixed ſubſtances, no part of them being elevated into vapour, in their calcination, except perhaps a ſmall portion of inflammable air, which eſcapes during that proceſs. Sulphur, oil, and alcohol, on the contrary, are wholly volatile on the application of fire. There are other ſubſtances, as wood and pit-coal, which, in the proceſs of inflammation, are partly fixed and partly volatile. We cannot, by comparing theſe ſubſtances with their aſhes, determine whether their capacities for containing heat are increaſed by combuſtion; becauſe the aſhes, which are the reſult of the combuſtion of a given quantity of them, bear a very ſmall proportion to the whole. Thus it appears, from the Experiments of the learned and ingenious Biſhop of Llandaff, that pit-coal contains one-third of its weight of aërial and liquid matters, which may be ſeparated by diſtillation; that dry heart of oak and ſallow loſe more than two-thirds of their weight in the ſame proceſs, and that from 106 pounds Avoirdupoiſe weight of dry peeled oak, only 19 ounces of aſhes were obtained. It is probable that a much ſmaller quantity would be produced by the combuſtion of an equal weight of green oak.

I have found that the aſhes which were obtained from 56 pounds of Newcaſtle coal, weighed ſeven pounds ſix ounces. Theſe aſhes were not compleatly calcined. If they had been expoſed to a ſtrong heat, in a crucible, it is likely that the diminution in their weight would have been much more conſiderable.

Doctor Prieſtley has proved that charcoal, when moiſtened with a little water, and heated to a ſufficient degree in cloſe veſſels, may be almoſt wholly reſolved into heavy inflammable air; a fluid which conſiſts partly of light inflammable air, and partly either of fixed air or of a ſubſtance that may be conſidered as its baſis.

If, therefore, the capacities of wood and pit-coal be found by experiment to exceed thoſe of their aſhes, we cannot from hence infer that the capacities of thoſe ſubſtances are diminiſhed by combuſtion. For it has been proved that the fixed matters in wood and pit-coal are combined with a large proportion of volatile principles, conſiſting chiefly of pure or of fixed air and water; and it appears, from experiment, that compounds, which conſiſt of theſe elements, united to principles that reſiſt the force of fire, have a greater capacity for heat than the latter in their uncombined ſtate. We find, for example, from the foregoing table, that the calces of metals, when they are ſaturated with air, contain more abſolute heat than the ſame ſubſtances, when the air is in a great meaſure extricated by nitrous acid, and expoſure to fire.

I have alſo found that chalk contains more abſolute heat than quicklime.

In order to determine the quantities of abſolute heat in chalk and quicklime, I had recourſe to alcohol as a ſtandard. For the comparative heat of quicklime could not be aſcertained with accuracy by making water the ſtandard, as theſe ſubſtances, when mixed together, produce much ſenſible heat; but when quicklime is mixed with alcohol, the heat produced during the firſt minute, is ſo ſmall as not materially to effect the reſult of the experiment.

The comparative heat of quicklime, as determined by this method, will appear from the following trials.

EXPERIMENT I.

Air in the room — 62.5.
Eleven ounces of powdered quicklime at — 66.2
mixed with 10.25 ounces of alcohol at — 139.

The mixture being agitated with the thin blade of a knife 20 times, its temperature

In 1 minute was — 119.75
2 — — 120.5
3 — — 121 plus
4 — — 121.75
5 — — 122.75
6 — — 123.75
7 — — 124.75
8 — — 125.5
9 — — 126.5
11 — — 128

In

In 16 minutes		—	131
21	—	—	134.5
31	—	—	137.5
41	—	—	140
51	—	—	134.5

EXPERIMENT II.

Equal bulks of quicklime, and the ſame alcohol, being mixed together at the temperature of 62, the mixture

In 1 minute was		—	62.2
2	—	—	62.4
3	—	—	62.5
4	—	—	62.6
5	—	—	62.7
6	—	—	62.8.

The ſpecific gravity of the alcohol uſed in theſe experiments was .784. Hence it appears, that more ſenſible heat is produced by mixing quicklime and alcohol when the latter is at a high temperature, than by mixing them when both are at a low temperature. It alſo appears, that, in the former inſtance, the quantity of ſenſible heat produced increaſes for a certain length of time, until it arrives at a maximum, after which it begins to diminiſh *.

Allowing ¾ of a degree for the ſenſible heat produced by the mixture, and one degree for that which was loſt by the agitation, we have ¼ of a degree for the abſolute quantity of heat ſeparated in the courſe of the firſt minute. This being added to 119.75 gives 120 for the true temperature of the mixture. The capacity of the veſſel was equal to that of .8 of an ounce of alcohol, the alcohol and veſſel were therefore equal to 11.05 ounces; the alcohol was cooled 19 degrees, and the quicklime heated 53.8. The comparative heat of quicklime is, therefore, to that of alcohol in the compound ratio of 11.05 to 11, and of 19 to 53.8, or as 1 to 2.8.

It was before ſhown that the comparative heat of chalk is to that of alcohol, as 1 to 2.34. Hence the comparative heat of chalk

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* It is manifeſt, that the production of ſenſible heat increaſed for a certain length of time in the above experiment. For though the aſcenſion of the mercury in the thermometer was equal during ſeveral minutes, yet as the temperature was augmented, the quantity of heat carried off by the air, in a given time, was continually encreaſing.

exceeds that of quicklime in the proportion of 2.8 to 2.34, or in that of 1.15 to 1.

Mr. Kirwan has proved that the mild volatile alkali contains more abſolute heat than the cauſtic. From the Experiments of this Philoſopher it alſo appears, that the dephlogiſticated nitrous and vitriolic acids have more abſolute heat than the ſame acids when they are not dephlogiſticated; and it is very probable that the dephlogiſtication of thoſe acids ariſes from the ſeparation of inflammable, and the abſorption of pure air. It moreover appears by experiment that the vitriolic acid, which probably derives its fluidity from water, contains more abſolute heat, when it is combined with a large proportion of that fluid, than when it is united to a ſmall proportion of it.

Hence we may conclude, in general, that when ſubſtances, which reſiſt the force of fire, are united to pure air, aërial acid, or water, the capacity of the compound is greater than that of the fixed ſubſtance, in its uncombined ſtate; and that the exceſs of the capacity of the compound, above that of the fixed ſubſtance, is, in ſome meaſure, proportional to the quantity of the fluids with which the latter is combined.

If this be admitted, it will follow that the capacities of wood and pit-coal, which are compounds conſiſting of fixed elements united to a large proportion of aërial and aqueous particles, muſt neceſſarily be greater than thoſe of the fixed parts that remain after theſe ſubſtances have been reſolved into their principles by combuſtion *.

I have endeavoured, by a variety of trials, to aſcertain the comparative heats of wood, of pit-coal, and of their aſhes.

In the following table I ſhall give the reader a brief view of the reſults of thoſe trials.

Duſt of the pine tree —	.50000
Charcoal — —	.26315
Aſhes of the ſame charcoal —	.09090
Aſhes of the elm tree —	.14025
Pit-coal — —	.27777
Cinders — —	.19230
Aſhes of cinders —	.18552

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* It is proper to obſerve, that the aſhes of vegetables are alſo combined with a portion of air and water. But the quantity of thoſe fluids, which is contained in the aſhes, is much ſmaller than that which exiſted in the vegetables previouſly to their combuſtion.

In making theſe experiments, the ſolid body, reduced to a fine powder, was raiſed in a ſlight tinned iron veſſel to the temperature of 139; the cold water was then ſuddenly poured upon it, and the veſſel was placed in ſuch a ſituation, that the ſurrounding air ſhould have nearly the ſame temperature with the mixture. Different parts of the ſame room were choſen for this purpoſe; the water, before it was mixed with the ſolid, being placed in the colder part of the room, and the mixture in the warmer.

By this contrivance the loſs of heat ariſing from the agitation, which was neceſſary to bring the parts of the mixture to a common temperature, was ſo ſmall as to produce a very ſlight inaccuracy in the experiment.

As the experiment were made in the winter ſeaſon, a ſufficient variation of temperature was readily obtained in the ſame room, becauſe the heat communicated by the ſolid to the cold water was not very conſiderable.

The ſolid was raiſed to the deſired temperature by placing it in the ſteam bath formerly deſcribed; and the heat of the mixture was meaſured by a thermometer that had each degree of Farhenheit's ſcale divided into ten equal parts.

The trials, from which the reſults contained in the foregoing table have been calculated, do not exactly correſpond with ſome experiments which were formerly communicated to the public on the ſame ſubject. But as they were ſeveral times repeated with great care, and were made in circumſtances which admit of a conſiderable degree of accuracy, I cannot help thinking that they approach near to the truth.

Theſe experiments prove that wood and pit-coal have more abſolute heat than their aſhes. We cannot however, as was before obſerved, infer from hence that the capacities of thoſe ſubſtances are diminiſhed by combuſtion, nor indeed can we conclude that the capacities of the fixed matters which they contain are diminiſhed by that proceſs. Notwithſtanding the above experiments, it is poſſible that the fixed matters in pit-coal and vegetables, may have a leſs capacity for heat when they exiſt in thoſe ſubſtances as conſtituent principles, than they have during their ſubſequent exiſtence in the ſtate of aſhes. Thus, if we conceive the capacities of the earthy and alkaline principles which exiſt in dry oak to be to that of the oak as 1 to 10, it will follow that the capacities of theſe principles are nearly doubled when they acquire the form of aſhes in the proceſs of combuſtion, becauſe it is found that the ratio of the capacity of the aſhes, produced by the burning of oak, is to that of the oak itſelf as 1 to 5.

If, however, we ſuppoſe, contrary to analogy, that the capacities of the aſhes of pit-coal and vegetables are diminiſhed by combuſtion; yet it is manifeſt that the ſum of the capacities of the principles, into which thoſe ſubſtances are reſolved by that proceſs, is increaſed. For fixed bodies, as was before ſhown, have a leſs capacity for heat than pure air, aërial acid, or water. When theſe ſubſtances enter into a chemical union with each other, it is very probable that the capacities of the fixed parts are increaſed, and that thoſe of the volatile parts are diminiſhed. We cannot, therefore, ſuppoſe that the fixed principles, which enter into the compoſition of wood, have a greater capacity for heat than the compound which conſiſts of thoſe principles combined with air and water. Let the capacities of the fixed principles be conceived, for the ſake of argument, to be equal to that of the compound. It has been ſhown that the capacity of dry oak is nearly five times greater than that of its aſhes. Hence it will follow, upon this ſuppoſition, that the fixed parts in dry oak, when they are changed into aſhes by combuſtion, undergo a diminution of capacity as 5; but it was proved that the fixed principles in dry oak compoſe not more than $\frac{1}{84}$ of the whole, the remaining 83 parts being, in the proceſs of combuſtion, reſolved into air and water. During this proceſs the water is raiſed into vapour, and the air is expanded into a permanently elaſtic fluid. The capacities of the air, and of the aqueous vapours, which are thus obtained, are at leaſt four times greater than that of dry oak. Conſequently, when this ſubſtance is reſolved into its principles by combuſtion, there is, in one part, a diminution of capacity as 5, and in 83 parts, an increaſe of capacity as 4; whence it appears that the ſum of the capacities of the principles into which dry oak is reſolved by that proceſs, is greatly increaſed. The ſame reaſoning will apply to the changes in the capacities of the elements derived from the combuſtion of other ſpecies of wood, as well as of pit-coal.

Hence we may conclude, in general, that the comparative heats of bodies, ſuppoſed to contain phlogiſton, are increaſed by the proceſſes of calcination and of combuſtion.

It follows, that when an inflammable body is deprived of its power of ſupporting flame, by the proceſs of combuſtion, it abſorbs a quantity of abſolute heat, and when, by a contrary proceſs, it again recovers its inflammability, an equal quantity of heat is detached. The calx of copper, for example, contains nearly twice as much abſolute heat as copper. When, therefore, by expoſing it to the action of fire, in contact with inflammable ſubſtances, the calx is revived, it will loſe one half of its abſolute heat; and, on the contrary, when the copper is again calcined, the calx will recover the heat which it had formerly loſt.

From the foregoing obſervations and experiments it appears, that the capacities of bodies are diminiſhed by their union with inflammable air, and increaſed by its ſeparation. Pure air, for example, by its union with that principle, is converted into water, and therefore its capacity for heat is diminiſhed. The experiments of Mr. Kirwan prove that the capacity of the vitriolic acid is alſo diminiſhed by phlogiſtication, and it is well known that this effect is produced by its union with inflammable air, or with ſubſtances containing that principle. It has moreover been ſhown, that by the peculiar combination of the heavy inflammable air with earth, and with water, in vegetables, and in pit-coal, the capacity of the compound for heat is diminiſhed.

Hence we may conclude, in general, that bodies have their capacities for heat leſſened by their union with inflammable air, and that when this principle is again detached, the elements, into which the body is reſolved, have their capacities for heat increaſed; from which it follows, that in the former inſtance a quantity of heat will be ſeparated from the body, and in the latter an equal quantity abſorbed. In this point of view the ſeparation of heat from a body, by means of the inflammable principle, and the re-abſorption of it, when that principle is again diſengaged, ſeems to be analogous to the ſeparation of fixed air from earths and alkalis by means of an acid; and the re-union of theſe ſubſtances with this fluid when the acid is ſeparated: if the vitriolic acid for inſtance be added to a mild alkali, the fixed air will be extricated, and will fly off in the form of an elaſtic vapour; if the inflammable principle be added to pure air, a portion of the abſolute heat will be ſeparated, and will fly off in the form of ſenſible heat; when the acid is again ſeparated from the alkali, the latter recovers the fixed air which it had loſt, and when the inflammable principle is again diſengaged from the pure air, the air re-abſorbs the heat which had formerly eſcaped from it.

PROPOSITION IV.

When an animal is placed in a warm medium, the colour of the venous blood approaches more nearly to that of the arterial, than when it is placed in a cold medium; the quantity of reſpirable air which it phlogiſticates, in a given time, in the former inſtance, is leſs than that which it phlogiſticates, during an equal ſpace of time, in the latter; and the quantity of heat produced, when a given portion of pure air is altered by the reſpiration of an animal, is nearly equal to that which is produced when the ſame quantity of air is altered by the burning of wax or charcoal.

That the difference between the colour of the venous and arterial blood, in a living animal, is diminiſhed by expoſure to heat, and increaſed by cold, appears from the following experiments.

EXPERIMENT I.

A dog, at 102, was immerſed in water at 114; ſo much of his head being uncovered as to allow him a free reſpiration.

In 5 minutes the dog was 108, water 112
6 dog 109, water 112
11 dog 108, water 112,
the reſpiration having become very rapid.
13 dog 108, water 112,
the reſpiration being ſtill more rapid.

In half an hour the dog was 109, water 112.

The animal was then in a very languid ſtate.

Small quantities of blood being drawn from an artery, and from a contiguous vein, the venous blood was found to have undergone a remarkable change of colour. For in the natural ſtate, the colour of the venous blood is a dark modena red; and that of the arterial a light ſcarlet. But after the animal, in the experiment in queſtion, had been immerſed in warm water for half an hour, the venous blood aſſumed very nearly the hue of the arterial, and reſembled it ſo much in appearance, that it was difficult to diſtinguiſh between them.

It is proper to obſerve that the animal, which was the ſubject of this experiment, had been previouſly weakened by loſing a conſiderable quantity of blood a few days before. When the experiment was repeated with dogs, which had not ſuffered a ſimilar evacuation, the change in the colour of the venous blood was more gradual; but in every inſtance in which the trial was made, and it was repeated ſix times, the alteration was ſo remarkable, that the blood which was taken in the warm bath, could readily be diſtinguiſhed from that which had been taken from the ſame vein before immerſion, by thoſe who were unacquainted with the circumſtances or motives of the experiment.

To diſcover whether a ſimilar change would be produced in the colour of the venous blood in hot air, a dog, at 102, was placed in air at 134. In 10 minutes the temperature of the dog was 104½, that of the air being 130. In 15 minutes dog 106, air 130. A ſmall quantity of blood was then taken from the jugular vein, the colour of which was ſenſibly altered, being much lighter than in the natural ſtate.

I next endeavoured to aſcertain the effect produced upon the colour of the venous blood, in a living animal, by expoſure to cold. To determine this, a dog, at 100, was immerſed in water at 45. In about a quarter of an hour a few ounces of blood were taken from the jugular vein, which was evidently much deeper in its colour than that which had been taken in the warm bath, and appeared to me, as well as to ſeveral other Gentlemen who aſſiſted in the experiment, to be the darkeſt venous blood we had ever ſeen.

From the foregoing experiments we may conclude, that when an animal is placed in a cold medium, the venous blood aſſumes a much darker hue than when it is placed in a warm medium.

Theſe experiments ſeem to confirm the following opinion, which was firſt ſuggeſted by my very ingenious friend Mr. Wilſon, of Glaſgow. Admitting that the ſenſible heat of animals depends upon the ſeparation of abſolute heat from the blood, by means of its union with the phlogiſtic principle in the minute veſſels, may there not be a certain temperature at which that fluid is no longer capable of combining with phlogiſton, and at which it muſt of courſe ceaſe to give off heat?

The following experiments prove that the air expired from the lungs of an animal, is more phlogiſticated in a cold than in a warm medium.

EXPERIMENT II.

The air in the room being at the temperature of 61, a ſmall Guinea pig was placed on a ſtand in an inverted glaſs jar that contained nearly five pints of common air, and was ſurrounded with water at 55½. It was ſuffered to remain in this ſituation 42 minutes, when it diſcovered ſigns of great uneaſineſs: being removed from the jar, it fell into convulſions, but ſoon recovered. The air in the jar was then tried by mixing two meaſures of it with one of nitrous air, and the meaſures of the teſt were found to be 2.7½ nearly; which ſhowed that it was about ¼ as pure as common air.

EXPERIMENT III.

A Guinea pig of nearly the ſame ſize being placed in the ſame jar, containing common air as before, and ſurrounded with water at 104, expired in 42 minutes nearly.

The air in the jar being then tried by mixing two meaſures of it with one of nitrous air, the teſt was a little more than 2.5, or it was nearly half as pure as common air. The phlogiſtication of the air, in the firſt experiment, was, therefore, twice as great as in the ſecond.

Hence it appears, that the degree of purity in the air, neceſſary to the ſupport of life, is greater when an animal is placed in a warm than in a cold medium; for in the ſecond experiment the animal ſurvived, though the purity of the air was diminiſhed three-fourths. In the laſt, it was only diminiſhed one half, and yet the animal expired.

It may likewiſe be proper to remark, that the ſymptoms of uneaſineſs diſcovered in the former experiment were much greater than in the latter. The eye in particular, in the one caſe, ſeemed to be much ſwoln, and became dim and languid; in the other, it retained its brightneſs to the laſt.

As it might be ſuſpected that the ſuperior purity of the air, in the latter experiment, did not ariſe from its having been reſpired at a higher temperature, but from ſome latent difference in the animals, the trials were repeated as follows.

EXPERIMENT IV.

A ſmall Guinea pig was placed in an inverted jar, containing nearly five pints of common air, which was ſurrounded with water at 46.5. The animal was ſuffered to remain in this ſituation 36 minutes, during which time it did not diſcover ſigns of much uneaſineſs. The air in the jar being then examined by means of nitrous air, the teſt was found to be 2.5, or it was one half as pure as common air.

EXPERIMENT V.

On the enſuing day the ſame Guinea pig, apparently in perfect health, was placed in the ſame jar ſurrounded with water at 102. It was removed at the end of 36 minutes without appearing to have received any injury. The air which remained in the jar being then examined by nitrous air, the teſt was found to be 2.2, or it was nearly $\frac{3}{4}$ as pure as common air.

As it might be imagined that, in theſe trials, a fallacy aroſe from the expoſure of the animal, in the firſt inſtance, to cold, and in the ſecond to hot air, the experiments were reverſed as follows.

EXPERIMENT VI.

A ſmall Guinea pig was placed in the ſame jar that was uſed in the former experiments. The jar was filled with common air as before, and ſurrounded with water at 102.

The Guinea pig being removed at the end of 36 minutes, and the air in the jar being examined by nitrous air, the teſt was 2.3 nearly.

EXPERIMENT VII.

The ſame animal, on the enſuing day, appearing to be in perfect health, was ſuffered to remain 36 minutes in the ſame jar, containing alſo common air, and ſurrounded with water at 52. The air being then examined as before, the teſt was 2.5.

From theſe experiments it appears, that when an animal is placed in a cold medium the quantity of air which it phlogiſticates, in a given time, is greater than when it is placed in a warm medium.

The following apparatus was contrived with a view to determine the quantities of heat produced, when given quantities of air were altered by the burning of combuſtible bodies, and by the reſpiration of animals. See plate IV. fig. 1.

A C repreſents a triple veſſel made of tinned iron, and conſiſting of an exterior veſſel A B C D, 16 inches wide, and 19.4 deep; a middle veſſel *e f g h*, 11¼ wide, and 13½ deep; and an interior veſſel *a b c d*, 9½ wide, and 10½ deep.

The middle veſſel is ſupported by a circular plate of iron, at the diſtance of 2.75 inches from the bottom of the exterior veſſel: and the diſtance of the cylindrical ſurface of the exterior, from that of the middle veſſel, is alſo 2.75 inches. The ſpace between theſe veſſels is ſtuffed with fine down.

The interior veſſel is ſupported by the pipes *h f*, *i g*, at the diſtance of .875 of an inch from the bottom of the middle veſſel, and this is likewiſe the diſtance of the cylindrical ſurfaces of the veſſels from each other.

The ſpace included between theſe veſſels is occaſionally filled with water.

In the upper ſurface of the interior veſſel there is an aperture five inches wide. To this aperture is ſoldered a flat ring of braſs *k l*, which is covered by a plate of the ſame metal, the ſurface of the latter being adjuſted to that of the former, with ſuch accuracy, that, when they are ſcrewed together, a piece of leather being interpoſed between them, they become air tight.

In the centre of the braſs plate there is a circular cavity an inch and a half wide, in which a thin plate of glaſs is cemented. By means of this glaſs the light of an inflamed body, placed in the centre of the apparatus, becomes viſible, and the focus of a lens may be occaſionally directed through it into the interior veſſel.

It was before obſerved that the interior veſſel reſts upon the pipes *h f*, *i g*. Theſe open into it at the points *h* and *i*. The latter being bent upward, extends along the interior ſurface of the middle veſſel, and when it has arrived at the point *n*, it is again bent at right angles, and terminates at *m*. To this extremity is ſcrewed the crooked pipe V Z, which is fixed by cement to the inverted jar E F, the latter being ſupported in the tub G H by the croſs bar Y O. The communication between the jar E F and the interior veſſel may be occaſionally interrupted by the ſtop cock V.

The pipe *h f* extends along the oppoſite ſide of the middle veſſel as far as the point *e*, where it is alſo bent at right angles, and terminates at *p*, in a conical braſs pipe, which enters into a ſimilar pipe at the extremity of the crooked tube R U. Theſe pipes are ground together with ſuch accuracy as to be air tight. They are cloſely applied to each other by means of a ſcrew.

The extremity R, of the crooked tube U R, is cemented into the neck of the inverted jar I K, which is placed in the tub L M; and a ſecond crooked tube S T paſſes from the neck of the ſame jar into the tub P Q, where it terminates under the inverted jar N O.

When an experiment is to be made with this apparatus, water is introduced into the tub G H till it riſes a little above the croſs bar Y O, on which the inverted jar E F reſts. The ſubject of the experiment is introduced into the interior veſſel *a c*, and the aperture *k l* is covered by the braſs plate, which is ſcrewed down in ſuch a manner as to be air tight. The ſpace included between the middle and interior veſſels is filled with water, and the upper ſurface of the exterior veſſel is cloſely covered with a thick cuſhion, ſtuffed with fine down.

The inverted jar I K, and the tub L M, are filled with water, and likewiſe the tub P Q, and the inverted jar N O. The cock

U being

U being ſhut, and V open, the diminution, which is produced in the air by the animal, or by the burning body, is meaſured by the riſing of the water in the jar E F.

The quantity of heat produced is determined by the increaſe of temperature in the water that ſurrounds the interior veſſel; and the eſcape of the heat from that fluid is prevented by the down with which it is encompaſſed.

The purity of the air in the interior veſſel is examined as follows: The cocks V and T are ſhut, and U opened. A part of the water is removed from the tub L M, in conſequence of which the water in the jar I K deſcends by its gravity; and in deſcending it draws a portion of air from the interior veſſel *a c*. The cock U is then ſhut, and T opened, and the water being returned into the veſſel L M, the air is forced by its preſſure from the jar I K into the jar N O, whence it is introduced into the eudiometer.

With this apparatus I made the following experiments.

EXPERIMENT VIII.

The barometer being 30.4, and the air in the room 67.5, a quantity of water weighing 31 pounds, ſeven ounces Troy, was taken at the temperature of 67.65; the ſpace included between the interior and middle veſſels was filled with a portion of this water to ſuch a height, that the upper ſurface of the fluid was nearly on a level with the mouth of the interior veſſel. A ſmall wax taper, which had been accurately weighed, was then lighted, and inſtantly placed in the centre of this veſſel; the mouth of which was now covered with the braſs plate, and the remaining part of the water was introduced. The ſurface of the latter roſe nearly one inch above the interior veſſel. The mouth of the exterior veſſel was covered with a cuſhion of down.

Before this experiment was made the jars E F and I K had been ſeparated from the veſſel A C, and the nozle of a pair of bellows had been fixed by means of a ſcrew to the extremity of the pipe *n m*. Freſh air was thus introduced into the interior veſſel, and the vitiated air eſcaped through the pipe *k f*. By this contrivance the taper was kept in a ſtate of inflammation for half an hour, when the experiment was diſcontinued.

The water, in the courſe of the experiment, was, at ſhort intervals, gently agitated by a wooden rod for the purpoſe of rendering the diffuſion of the heat uniform; and the temperature of the air in the room was augmented nearly in the ſame degree with that of the water. The heat of the water was meaſured by a thermometer, that had each degree of Farhenheit's ſcale divided into ten equal parts.

At the end of the experiment the thermometer, in the water, ſtood at 69.75, the air in the room being 69.5 nearly. The loſs of weight in the wax taper was 26 grains.

Thus it appears, that by the combuſtion of 26 grains of wax, the temperature of 31 pounds ſeven ounces of water was raiſed 2.1 degrees.

To diſcover how much air was altered by the combuſtion of this quantity of wax, I made the following experiment.

EXPERIMENT IX.

A large tub was ſilled with lime water. A piece of cork, which had a portion of the ſame wax taper that was uſed in the foregoing experiment fixed in its centre, was made to ſwim on the ſurface of this water. Four narrow ſlips of thin paper, applied to each other by their flat ſurfaces, were tied to the upper extremity of the taper, being of ſuch a breadth as to ſurround one half of the wick. A ſmaller number of ſlips than four did not furniſh a ſufficient heat to kindle it; and it was found that the wick could not readily be inflamed unleſs a part of its ſurface were expoſed to the air. The ſlips were made very dry by preſſing them with a hot iron. They weighed a little leſs than a grain.

Over this taper a large cylindrical glaſs veſſel was inverted, which had a ſcale of equal parts affixed to it. A tube of a conſiderable length, that was nearly bent double, had one of its extremities introduced into the inſide of this veſſel, the other extremity riſing above the ſurface of the water on the outſide. The veſſel was now preſſed into the water, and the greater part of the air which it contained eſcaped through the tube, which was then withdrawn, and pure dephlogiſticated air introduced in ſuch quantity, that the ſurface of the water, in the inſide of the jar, ſtood at the height of three inches from its inverted mouth.

For the purpoſe of determining the height of the water with greater exactneſs, the jar was removed from the tub, and was placed upon a table in a ſhallow veſſel, the depth of which was little more than three inches; by this means the eye could be brought nearly to the ſame level with the point which was to be obſerved, and the ſurface of the water was rendered diſtinct by the reflection of the light from the bottom of the veſſel. When this obſervation was made, one candle was placed behind the jar, and another before it; by the latter, the diviſions of the ſcale were illuminated, and by the former, the light was thrown upon the ſurface of the water.

It is proper to obſerve, that when the point at which the water ſtood was marked, the fluid, which ſurrounded the outſide of the jar, was raiſed to the ſame height with that in the inſide, in conſequence of which the air in the jar had the ſame denſity with the external air; and the jar was kept in an horizontal poſition by means of a *ſpirit level.*

The point at which the water ſtood in the jar being thus accurately determined, the taper was then inflamed by throwing the focus of a lens upon the ſlips of paper that were fixed to its upper extremity, and was allowed to burn until the air in the jar was ſo far vitiated, as to be no longer capable of ſupporting the inflammation. It was now removed from the jar by making it to paſs under the ſurface of the water, and having been carefully dried, it was weighed, when it was found to have loſt eleven grains. The air in the jar was then introduced into large bottles, which were half filled with lime water, and being agitated until the fixed air was abſorbed, the reſiduum was returned into the jar, which was placed in a veſſel nearly three inches deeper than that formerly uſed, and the ſurrounding water being brought to a level with that in the jar, the internal ſurface ſtood at 5.75 inches. The diminution of the air was, therefore, ſuch as to raiſe the water from 3 to 5.75, or 2.75 inches. The quantity of water which occupied one inch in this jar was 15 ounces Troy. Hence the quantity of pure air, altered by the combuſtion of eleven grains of wax, was 41.25 ounce meaſures, or 78.2 cubic inches; and conſequently, the quantity of air changed by the combuſtion of 26 grains of wax, was 97.5 ounce meaſures, or 184.84 cubic inches. When this experiment was made the thermometer, which meaſured the heat of the air in the room, was at - 70

The barometer at - 30.4.

It is of importance that the portions of wax employed in theſe experiments, ſhould reſemble each other as nearly as poſſible in their purity; and, therefore, parts of the ſame taper were uſed in both experiments. When the trials were repeated with different tapers, a variation was obſerved in the reſults: thus I found that when a ſpecies of wax, ſomewhat purer than that employed in the foregoing experiments, was inflamed in the above deſcribed apparatus, and was ſuffered to burn until the water was raiſed 2.15 degrees, the loſs of weight in the wax was only 22.5 grains.

6.75 grains of this wax being burned in dephlogiſticated air, the diminution of the latter was nearly 30 ounce meaſures Troy, which gives 100.7 ounce meaſures for the quantity of air altered by the combuſtion of 22.5 grains.

EXPERIMENT X.

To determine the quantity of heat produced by an animal, a *Guinea pig* was introduced into the centre of the interior veſſel deſcribed above, which was ſurrounded by 31 pounds ſeven ounces Troy of water, at

the temperature of -	62
In half an hour the water being brought by agitation to an uniform heat, its temperature was - -	62.25
In an hour it was -	62.5
In an hour and a half -	62.75
In two hours - -	63.

The heat of the air in the room at the beginning of the experiment was 62; and its temperature was augmented nearly in the ſame degree with that of the water.

Hence the heat imparted to the water by the animal in two hours was 10 degrees by my thermometer, or one degree by Farhenheit's.

The quantity of air altered by the reſpiration of the animal, in that time, was determined as follows.

EXPERIMENT XI.

Part I. (ſee plate 4, fig. 2.)

The ſame *Guinea pig* was introduced into the glaſs jar A B, which was inſtantly inverted over water in the tub C D, and was made to reſt upon the croſs bar E F, the animal being ſupported by the ſtand G H. One extremity I, of the crooked tube K L M N O, was made to paſs through the croſs bar E F into the jar A B, where it roſe a little way above the ſurface of the water: the other extremity P terminated under the mouth of the inverted jar Q R, which was filled with water, and was ſupported by a croſs bar in the tub S T. As ſoon as the jar A B, containing the animal, was inverted over the water, a greater quantity of that fluid was introduced into the tub, by the preſſure of which a part of the air was forced from the jar through the crooked tube, and was received in the veſſel Q R. The animal was ſuffered to remain in the jar 35 minutes; it was then removed by making it to paſs under the ſurface of the water.

The air in the jar A B was agitated in large bottles with lime water until the fixed air was abſorbed, and likewiſe that which had been received in the veſſel Q R. The whole was then returned into the veſſel A B, and the height of the water being determined with the precautions mentioned above, it was found to ſtand at 3.312 inches. When this experiment was made the air in the room was - - - 62.5.

The barometer - - 30.

In the experiment formerly related, that was made with a view to determine the quantity of air altered by the combuſtion of wax, the thermometer was at 70,
the barometer at - 30.4.

The variation in the heights of the barometer, in theſe experiments, was therefore $\frac{4}{10}$ of an inch. Hence, if the temperature of the air in both inſtances had been the ſame, the denſities would have been as 76 to 75; and conſequently the bulk of the air in Experiments VIII. and IX. in order to reduce it to the ſame denſity with the air in the Xth Experiment, muſt have been increaſed $\frac{1}{76}$ of the whole, or 1.3 ounce meaſures. But the temperatures varied from each other by 7.5 degrees. Suppoſing the air to be expanded $\frac{1}{483}$ of its bulk by one degree of heat, it follows, that if the heights of the barometer had been the ſame, the denſities would have been as 64 to 65 nearly; or the bulk of the air in the preceding trials, in order to reduce it to the ſame denſity with the air in the laſt experiment, muſt have been diminiſhed by $\frac{1}{65}$ of the whole, or 1.4 ounce meaſures. It was before ſhown that it muſt be increaſed 1.3 ounce meaſures on account of the difference in the heights of the barometer. If, therefore, in thoſe experiments the barometer had ſtood at 30, and the thermometer at 62.5; the bulk of the air changed by the combuſtion would have been 97.5 + 1.3 — 1.4, or 97.4 ounce meaſures.

EXPERIMENT XI. Part II.

The Guinea pig being introduced into the ſame jar, the latter was inverted over water; a part of the air was forced from it as before;

the animal was immediately removed by making it to paſs under the ſurface of the water; and the air that was expelled was again returned into the jar, when it was found that the water in the jar ſtood at 2.187 inches. Hence the diminution of the air by the reſpiration of the animal during 35 minutes, was ſuch as to raiſe the water in the jar from 2.187 to 3.312 inches, or 1.125 inches.

As the quantity of water which occupied the ſpace of one inch in this jar was 15 ounces Troy, it follows that, in the experiment in queſtion, the air was diminiſhed 16.875 ounce meaſures Troy, and conſequently the quantity of pure air, altered by the reſpiration of the animal, in two hours, was 57.8 ounce meaſures Troy, or 109.5 cubic inches.

When I firſt tried this experiment, I introduced the animal into the inverted jar, by making it to paſs under the ſurface of the water; but the reſult, in theſe circumſtances, was liable to a conſiderable degree of inaccuracy. For when the animal is dry it alters a ſmaller quantity of air in a given time than when it is wet. In the latter inſtance cold is produced by the evaporation from the ſurface of the animal, and by the experiments recited in page 312, it is proved that when an animal is placed in a cold medium, it phlogiſticates a greater quantity of air, in a given time, than when it is placed in a warm medium.

I therefore found it neceſſary firſt to introduce the animal into the jar, and then, as ſpeedily as poſſible, to invert the latter over water, the animal being ſupported by the ſtand as deſcribed above.

When the experiment is made in this way, a ſlight inaccuracy ariſes from the rarefaction of the air, by the heat which the animal communicates, during the ſhort interval that elapſes between its introduction into the jar, and the inverſion of the latter over water; but this inaccuracy was corrected by Experiment IV. part 2. For when the Guinea pig was removed, in that experiment, the aſcenſion of the water in the jar meaſured not only the ſpace which the animal had occupied, but alſo the air that was expelled by the heat which it communicated.

It has been proved that the quantity of pure air, altered by the reſpiration of the animal, in two hours was 57.8 ounce meaſures Troy, and that the quantity of heat imparted to the water during the ſame period of time was one degree.

Hence it follows, that if an increaſe had been produced in the temperature of the water, amounting to 2.1 degrees, the quantity of air changed would have been 121 ounce meaſures. It was before ſhown, that when an equal quantity of heat was produced by the burning of a wax taper, the quantity of air altered was nearly 100 ounce meaſures.

A ſeries of experiments, ſimilar to thoſe now recited, have been made by Meſſrs. Lavoiſier and De la Place with a different apparatus. See Memoire ſur La Chaluir, Lû a l'Académie Royale des Sciences, le 28 Juin, 1783.

According to the experiments related in this Memoire, when equal portions of air were altered by a Guinea pig, and by the combuſtion of charcoal, the quantity of heat excited in the former inſtance was to that in the latter as 13 to 10.3 nearly. It is proper to obſerve that, in thoſe experiments, the quantity of air altered by the reſpiration of the animal was meaſured, by placing it in air which had nearly the temperature of 60; and when the heat which it produced was determined, it was placed in a medium which had the temperature of 32 or 33. Now it has been proved, that when an animal is ſurrounded by a medium at a low temperature, it phlogiſticates a greater quantity of air, in a given time, than when it is ſurrounded by warm medium. Hence we may conclude, that the quantity of air phlogiſticated by the animal, in Mr. Lavoiſier's apparatus for meaſuring heat, was greater than that which has been determined by his calculation. This cauſe of inaccuracy was ſuſpected by the Authors of the Memoire.

I have repeated Mr. Lavoiſier's experiments with charcoal, and have obtained the following reſults.

EXPERIMENT XII.

An oblong piece of charcoal was taken which weighed 73.8 grains. It was formed in ſuch a manner that one of its extremities ſhould taper until it nearly terminated in a point. The air having been expelled from it by expoſure to heat, its conical extremity was ſet on fire by applying it to the flame of a candle, upon which it was inſtantly placed in the centre of the apparatus employed for meaſuring heat. Thus ſituated, it was kept in a ſtate of inflammation by a current of freſh air, which was introduced into the interior veſſel by means of the bellows and pipe deſcribed above.

The interior veſſel was ſurrounded by 31 lb. 7 ounces Troy of water, which, at the beginning of the experiment, had the temperature of 62.7.

The air in the room was 62.8. At the end of 22 minutes, the water being reduced by agitation to a uniform heat, its temperature was 65.9. The air in the room 66 nearly.

The charcoal after the combuſtion weighed 17.8 grains. The loſs of weight was, therefore, 56 grains.

EXPERIMENT XIII.

Air in the room -	61.8
Barometer - -	30.

The upper ſurface of a circular piece of cork, ſimilar to that mentioned in Experiment IX, was covered with a thin plate of tinned iron, to which two ſmall perpendicular plates of the ſame metal were fixed at a little diſtance from the centre. Between theſe plates an oblong piece of charcoal, weighing 91.1 grains, was placed; the diameter of the latter, compared with the diſtance of the plates from each other, being ſuch, that when it was ſlipped in between them, it was retained in its proper poſition by their elaſticity.

The charcoal thus connected with the cork was made to ſwim on the ſurface of lime water; and the jar, which was uſed in the former experiments, was inverted over it, the common air being expelled, and dephlogiſticated air introduced in the manner deſcribed in Experiment IX.

The height of the water in the jar, determined with the precautions mentioned above, was found to be 3.125 inches. The charcoal was then ſet on fire by a lens, and was ſuffered to burn until it was extinguiſhed in conſequence of the contamination of the air; after which the charcoal was removed by making it to paſs under the ſurface of the water, and was perfectly dried, when it was found to have loſt 14 grains. The air that remained in the jar was introduced into large bottles, and was agitated with lime water; and being again returned into the ſame jar, the water in the latter was found to ſtand at 5.75 inches. The diminution of the air was, therefore, 39.375 ounce meaſures Troy, or 74.65 cubic inches. With a view to avoid the inaccuracy which might ariſe from a difference in the qualities of different kinds of charcoal, portions of the ſame piece were uſed in both theſe experiments.

The laſt trial was repeated with the following reſult.

EXPERIMENT XIV.

Air in the room - -	61
Barometer - - -	29.6

A piece of charcoal, which, when very hot, weighed one dram and 27¾ grains, was made to ſwim upon lime water. The jar being inverted over it as before, and a quantity of dephlogiſticated air introduced, the water was found to ſtand at 3.064 inches. The charcoal was now ſet on fire by a lens, and was ſuffered to burn till the air was ſo much vitiated as to be incapable of ſupporting the inflammation.

The reſiduum of the coal was then removed by making it to paſs under the ſurface of the water. Being accurately dried and made very hot, it was again weighed, when it was found to have loſt 12½ grains.

The air which remained in the jar was agitated as before with lime-water, and was returned into the jar, when the water in the latter ſtood at 5.625 inches.

Hence it appears, that after the agitation with lime-water, the volume of the air in the jar was reduced 2.561 inches. The quantity of pure air that was altered was, therefore, 38.4 ounce meaſures troy, or 72.4 cubic inches.

To determine with certainty whether any fixed air was mixed with the air that remained in the jar, a portion of the latter, at the end of the experiment, was agitated with lime-water, when it was found that no precipitation took place.

Reducing the air to the ſame denſity in the two preceding experiments, and taking the mean reſult, we have 41.29 ounce meaſures, or 78.3 cubic inches for the quantity of pure air altered by the combuſtion of 14 grains of charcoal, and hence 107.8 cubic inches would be altered by the combuſtion of 18 grains.

According to Mr. Lavoiſier's experiments, 128.38 French cubic inches were altered by the combuſtion of an equal quantity of that ſubſtance; the barometer ſtanding at 28 inches, and the thermometer at 54.5. If the barometer had ſtood at 30, which was nearly its mean height in the above experiments, and the thermometer at 61; 121.34 French, or 138.32 Engliſh cubic inches would have been altered. There is, therefore, a difference of 30.5 cubic inches between the reſults of the foregoing trials, and thoſe of Meſſrs. Lavoiſier and De la Place.

It is poſſible that this variation may have partly ariſen from a difference in the qualities of the portions of charcoal that were employed. The coal which I uſed was charred oak.

From Experiments XIII. and XIV. we learn, that 41.29 ounce meaſures of pure air were altered by the combuſtion of 14 grains of charcoal. Conſequently, by the combuſtion of 56 grains, 165.16 ounce meaſures will be altered. And from Experiment XII. we learn, that the quantity of heat communicated by the burning of 56 grains of charcoal, to 31 lb. 7 oz. of water, was 3.2 degrees. Whence it follows, that, in order to the communication of 2.1 degrees of heat, it would be neceſſary that 108.3 ounce meaſures of pure air, ſhould be altered by the combuſtion of charcoal. And we have ſeen, that an equal quantity of heat was communicated to the water, when 100 ounce meaſures of pure air were altered by the burning of wax.

It is proper to obſerve, that when the wax taper was burned in the above deſcribed apparatus, with a view to determine the heat which it produced, a conſiderable quantity of aqueous vapour was condenſed on the inner ſurface of the interior veſſel. But no vapour appeared when the experiment was made with charcoal. It is therefore probable that, in the former inſtance, a part of the pure air was converted into water; and that in the latter, it was principally, if not entirely, changed into fixed air. This concluſion is, I think, confirmed by the following experiments.

EXPERIMENT XV.

Air in the room - -	67
Barometer - - -	29.5

The jar being inverted as before, over a wax taper, which was made to ſwim on the ſurface of pure water, a quantity of dephlogiſticated air was introduced, when the water in the jar was found to ſtand at 3.312 inches.

The taper was now kindled, and ſuffered to burn till it was extinguiſhed in conſequence of the deficiency of pure air, and the jar being ſpeedily cooled to the temperature of the ambient medium, the water, previouſly to the abſorption of the fixed air, was found to ſtand at 4.187 inches. After the fixed air was abſorbed by agitating it with lime-water, and the remaining air was returned into the jar, the water in the latter ſtood at 6 inches.

The diminution of the air, therefore, by the combuſtion of the wax, previouſly to the abſorption of the fixed air, was to the entire diminution, after the fixed air was abſorbed, as .875 to 2.688, or as 1 to 3.07.

EXPERIMENT XVI.

Air in the room - -	61
Barometer - - -	29.6

A piece of charcoal was made to ſwim on lime-water; the jar was inverted over it; and a quantity of dephlogiſticated air was introduced, by which the water in the jar was depreſſed until it ſtood at 3.064 inches. The charcoal was now ſet on fire by a lens, and was ſuffered to burn till it was extinguiſhed in conſequence of the contamination of the air.

The air in the jar being brought to the temperature of the ſurrounding atmoſphere, the water was found to ſtand at 3.439 inches. The fixed air was then abſorbed by agitating it, in bottles, with lime-water; and the remaining air being returned into the jar, the water in the latter ſtood at 5.625 inches.

Hence the diminution of the air by the combuſtion of the charcoal, previouſly to the abſorption of the fixed air, was to the entire diminution, as .365 to 2.561, or as 1 to 7.01 nearly.

As I was apprehenſive, that in the laſt experiment, the air in the jar had not been completely cooled to the temperature of the ambient medium, it was repeated with the following reſult.

EXPERIMENT XVII.

Air in the room	-	-		62
Barometer	-	-	-	29.8

The jar being inverted over a piece of charcoal, which was made to ſwim on the ſurface of lime-water, and dephlogiſticated air being introduced as before, the water was found to ſtand at 3.562 inches.

The charcoal was then kindled, and ſuffered to burn till the pure air was vitiated, and the jar was cooled to the temperature of the ſurrounding air, by pouring cold water over it. With a view to ſatisfy myſelf that it had acquired the exact temperature of the ſurrounding air, I ſuffered it to remain in a ſtate of reſt during the ſpace of 2½ hours; the air in the room retaining very nearly the ſame temperature that it had at the beginning of the experiment. The water in the jar was now found to ſtand at 3.875 inches. After the fixed air was abſorbed, it ſtood at 5.75 inches.

The diminution of the air in this experiment, previouſly to the abſorption of the fixed air, was to the entire diminution, as .313 to 2.188, or nearly as 1 to 6.99. In the foregoing experiment, it was as 1 to 7.01. And taking the mean reſult, it will be as 1 to 7 very nearly*.

It was before ſhewn, that the diminution which was produced in the volume of the pure air, by the combuſtion of wax, was to the entire diminution after the fixed air was abſorbed, as 1 to 3 nearly.

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* The mean reſult of the foregoing experiments, gives 67.3 cubic inches for the volume of the pure air changed by the combuſtion of the charcoal, and 57.7 for the volume of the fixed air which was produced in that proceſs. The mean height of the barometer was 29.55 inches; and the mean temperature was 61.5. If the barometer had ſtood at 28, and the thermometer at 54.5, the volumes would have been 70.26 and 59.68. Suppoſing that when the barometer is at 28, and the thermometer at 54.5, a cubic inch of pure air weighs .32105 of a grain, Troy, and a cubic inch of fixed air .47489 of a grain; the weight of 70.26 cubic inches of the former will be 22.55 grains, and that of 59.68 of the latter, 28.34. It is manifeſt that the exceſs of the weight of the fixed, above that of the pure air, muſt have ariſen from its being combined with a principle furniſhed by the charcoal; and conſequently, the quantity of the inflammable principle contained in fixed air will be nearly $\frac{1}{5}$th of the whole. According to Mr. Lavoiſier's experiment, it is only $\frac{1}{9}$th of the whole. As theſe proportions vary from each other very much, I repeated my experiments with great care, but I could not find the reſults to be materially different from thoſe which have been ſtated above.

From theſe experiments, compared with the obſervations recited in page 339, it follows, that when pure air is phlogiſticated by the burning of wax, it is partly converted into aqueous vapour, and partly into fixed air; and that when it is phlogiſticated by the burning of charcoal, it is entirely converted into the latter fluid.

As the vapour which is the reſult of the former proceſs is ſuddenly condenſed, the air that remains in the jar, previouſly to the abſorption of the fixed air, ſuffers a much greater diminution of bulk, than it does in thoſe caſes in which no aqueous vapour is produced.

I have found that the combuſtion of tallow and oil, as well as that of wax, is accompanied with the production of aqueous vapour. That a quantity of water is produced by the combuſtion of oil, is alſo proved by the experiments of Mr. De Luc.

From theſe facts it ſeems extremely probable, that oil, wax and tallow, and moſt other inflammable ſubſtances, contain two kinds of inflammable air, one of which reſembles the light inflammable air of metals, and the other, the heavy inflammable air obtained from vegetables by expoſure to heat in cloſe veſſels. The firſt, by its union with pure air, produces water; and the laſt, fixed air; and hence fixed air and water are the reſult of the combuſtion of wax and oil. When thoſe ſubſtances are reduced to a coal, by expoſure to heat in cloſe veſſels, it ſeems probable that the light inflammable air, combined with the volatile parts, eſcapes, and that the heavy inflammable air is retained; and, conſequently, fixed air alone is produced by the combuſtion of charcoal.

Having found that the pure air which is altered by the burning of wax, oil, and tallow, is partly converted into water, and partly into fixed air, I next endeavoured to determine whether a ſimilar change is produced by the reſpiration of an animal.

EXPERIMENT XVIII.

Air in the room - -	67
Barometer - - -	30

A Guinea pig was introduced into a glaſs jar, containing dephlogiſticated air, which was inverted over water, the mouth of the jar being cloſed with a cork to prevent the abſorption of the fixed air. The animal was ſuffered to remain in this ſituation a little more than an hour, when the cork was removed, and the water permitted to enter. The latter now occupied the ſpace of 16 cubic inches in the jar. The Guinea pig was then withdrawn by making it to paſs under the ſurface of the water, and the point at which the water ſtood in the jar was accurately determined. The experiment being diſcontinued for a time, the height of the water was again obſerved at the end of eight hours, when it was found to have remained nearly ſtationary; from which it appears that fixed air is very ſlowly abſorbed by water when they are both in a ſtate of reſt.

The air being now agitated with lime water, the reſiduum was returned into the jar; and by the aſcenſion of the water it was found that 40.86 cubic inches of fixed air were abſorbed.

It was before ſhown that the diminution of the air produced by the reſpiration of the animal alone, was 16 cubic inches. It now appears that the diminution produced by the abſorption of the fixed air was 40.86 cubic inches; the entire diminution, therefore, or the quantity of air altered, was 56.86 cubic inches, or 18.9 grains.

Hence it follows that part of the pure air, changed in the above experiment, was converted into water; for by the experiment with charcoal it is proved that, if the pure air had been entirely converted into fixed air, the diminution which it would have ſuffered, previouſly to the agitation with the lime water, would not have been more than the $\frac{1}{7}$ of the whole; but, in the experiment in queſtion, it was nearly $\frac{1}{3.5}$ of the whole. A part of the air, therefore, in the latter inſtance, muſt have been converted into aqueous vapour, which was condenſed upon the internal ſurface of the jar. It moreover appears, from the trials with charcoal, that if the pure air had been entirely converted into fixed air, the weight of the latter would have been $\frac{1}{5}$ greater than that of the former; but in the laſt mentioned experiment, the weight of the pure air altered by the reſpiration of the animal, was nearly equal to that of the fixed air which was produced by this proceſs; for the weight of the pure air was 18.9 grains, and that of the fixed air 20.1 grains. Of the latter quantity $\frac{4}{5}$ conſiſted of pure air, and $\frac{1}{5}$ of heavy inflammable air; that is the quantity of pure air, changed into fixed air, was 16.08 grains; but the entire quantity of pure air, altered during the proceſs, was 18.9 grains. It follows that 2.82 grains of this fluid were changed into water, and 16.08 grains into fixed air.

From the XVIth Experiment it is manifeſt, that fixed air conſiſts of pure air, and of a principle furniſhed by inflammable ſubſtances, agreeably to the opinion of Dr. Black, Mr. Kirwan, and Mr. Lavoiſier. For the quantity of fixed air produced, in that experiment, was 28.34 grains, (ſee note, p. 343) the quantity of pure air altered by the proceſs was 22.55 grains, and the quantity of charcoal conſumed was 12.5 grains.

Hence it is not poſſible that the fixed air could have been ſolely derived from the charcoal, or from the pure air, becauſe its weight exceeded that of either of thoſe ſubſtances; and that it could not have been derived from the water, appears from the reſult of ſimilar experiments which have been made over mercury.

Since, therefore, during the combuſtion of charcoal, a quantity of fixed air is produced, greater than the entire quantity, either of coal or of pure air that diſappear, we cannot, I think, avoid concluding, that the fixed air is formed by an union between the pure air, and a principle furniſhed by the charcoal.

It moreover appears from Experiments VIII. and IX. that when wax is burned in pure air, more than the one half of it enters into the compoſition of the fixed air and aqueous vapour, which are produced during that proceſs. For the volume of the pure air, altered in Experiment VIII. was 184.84 cubic inches; the barometer ſtanding at 30.4, and the thermometer at 70. If the former had ſtood at 28, and the latter at 54.5, the volume would have been 194.44 cubic inches, or 62.4 grains. The bulk of the pure air, previouſly to the abſorption of the fixed air, was diminiſhed one third in conſequence of the combuſtion of the wax *. The quantity of fixed air produced was, therefore, 129.6 cubic inches, or 61.5 grains.

We have ſeen that when pure air is converted into fixed air, its bulk is diminiſh-

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* See Experiment XV.

ed $\frac{1}{7}$ of the whole; therefore the volume of the pure air contained in the above-mentioned 129.6 cubic inches of fixed air, muſt have been 148.2 cubic inches; ſubtracting this from 194.4, we have 46.2 cubic inches, or 14.8 grains, for the quantity of pure air which was converted into water. The fixed air contained $\frac{1}{5}$ of its weight of inflammable matter, and the water nearly $\frac{1}{6}$; and hence the quantity of inflammable matter, which entered into the compoſition of the fixed air and water, was 14.78 grains. From which it appears that wax conſiſts principally of heavy and of light inflammable air, in a ſolid ſtate, ſince the greater part of it is combined with the pure air which is altered during its combuſtion.

The following table exhibits the quantities of heat communicated to 31 pounds ſeven ounces Troy of water, by the combuſtion of different ſubſtances in the apparatus deſcribed above *.

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* The thermometer employed for meaſuring the heat expreſſed in this, and in the ſubſequent table, had the degrees of Farhenheit's ſcale ſubdivided into decimals; and, conſequently, each degree correſponds with the $\frac{1}{10}$ of a degree of Farhenheit.

Degrees

	Degrees.
The heat imparted by the combuſtion of half a dram of wax was -	24.2
By the combuſtion of half a dram of tallow - - -	24.
By the combuſtion of half a dram of the beſt lamp oil -	22.3
By the combuſtion of half a dram of charcoal - -	17.1

When equal quantities of pure air were altered by the combuſtion of wax and charcoal, and by the reſpiration of an animal, the quantities of heat communicated to 31 pounds ſeven ounces of water were as follows.

		Degrees.
100 ounce meaſures of pure air, altered	by the combuſtion of wax, imparted	21.
	by the combuſtion of charcoal -	19.3
	by the reſpiration of a Guinea pig	17.3

From the foregoing experiments it appears, that the quantities of air altered being equal, more heat is produced by the combuſtion of wax than by that of charcoal, and more by the combuſtion of charcoal than by the reſpiration of an animal. Theſe differences probably ariſe from the following cauſes: in the firſt inſtance, a conſiderable part of the air is converted into water, and more heat is produced by the converſion of pure air into that fluid than into fixed air. In the laſt inſtance, a part of the heat is carried off by the inſenſible perſpiration.

The reſults, however, approach ſo nearly to each other as to prove that the heat, in theſe proceſſes, ariſes principally, if not entirely, from the converſion of pure air into fixed air, or into water.

Upon the whole we have ſufficient evidence for concluding, that when an animal is placed in a warm medium, the colour of the venous blood approaches more nearly to that of the arterial than when it is placed in a cold medium; that the quantity of reſpirable air, which the animal phlogiſticates in a given time, in the former inſtance, is leſs than that which it phlogiſticates, during an equal ſpace of time, in the latter; and that the quantity of heat produced, when a given portion of pure air is altered by the reſpiration of an animal, is nearly equal to that which is produced, when the ſame quantity of air is altered by the combuſtion of wax or charcoal.

SECTION III.

From the facts which have been eſtabliſhed by the above experiments, the following explanation may be given of animal heat, and of the heat which is produced by the inflammation of combuſtible bodies.

1. *Of Animal Heat.*

It has been proved, that the air which is received into the lungs of animals, contains more abſolute heat than that which is exhaled in expiration. It has been ſhown, particularly, that nearly $\frac{1}{6}$ of the pure air which is altered by the proceſs of reſpiration, is converted into aqueous vapour, and the remaining $\frac{5}{6}$ into fixed air; that when a given quantity of pure air is changed into fixed air, in phlogiſtic proceſſes, the weight of the latter exceeds that of the former by $\frac{1}{5}$ of the whole; and that, when the quantities of matter in theſe fluids are equal, the comparative quantities of heat are as 4 to 1; from which it appears, that the quantity of matter in the fixed air being to that in the pure air as 5 to 4, the quantity of heat in the latter will be to that in the former as 3 to 1 nearly; and it has been proved, that this is likewiſe the ratio of the heat of pure air to that of aqueous vapour.

Since, therefore, the fixed air and aqueous vapour, which are exhaled by expiration, are found to contain only the $\frac{1}{3}$ part of the heat which was contained in the purer portion of the atmoſpherical air, previouſly to inſpiration, it follows that the latter muſt neceſſarily give off a conſiderable proportion of its abſolute heat in the lungs.

It has moreover been ſhown, that the comparative heat of florid arterial blood is to that of venous as 11½ to 10; and hence, as the blood which is returned by the pulmonary vein to the heart, has the quantity of its abſolute heat increaſed, it is evident that it muſt have acquired this heat in its paſſage through the lungs. We may conclude, therefore, that, in the proceſs of reſpiration, a quantity of abſolute heat is ſeparated from the air, and abſorbed by the blood.

The truth of this concluſion will perhaps appear in a clearer light from the following calculation, by which we may form an eſtimate of the quantity of heat yielded by the purer part of atmoſpherical air, when it is converted into fixed air, and aqueous vapour; and alſo of that which is abſorbed during the converſion of venous into arterial blood.

We have ſeen, that the comparative heat of pure air is to that of the fixed air and aqueous vapour, into which it is changed in the lungs, as 3 to 1. The ſame heat, therefore, which raiſes the pure air one degree, will raiſe the fixed air and aqueous vapour three degrees; and, conſequently, the ſame heat which raiſes pure air, any given number of degrees, will raiſe the fixed air and aqueous vapour the ſame number of degrees multiplied by three.

It has been proved that pure air, when at the common temperature of the atmoſphere, contains 1550 degrees of heat. Hence if a certain quantity of pure air, not in contact with any body that would immediately carry off the heat, ſhould ſuddenly be converted into fixed air and aqueous vapour, the heat which was contained in the former would raiſe the latter 1550 degrees multiplied by three, or 4650 degrees; and the temperature of red hot iron being 1050, it follows that the quantity of heat, which is yielded by pure air, when it is converted into fixed air and aqueous vapour, is ſuch (if it were not diſſipated) as would raiſe the air and vapour, ſo changed, to more than four times the exceſs of the heat of red hot iron above the common temperature of the atmoſphere.

If, therefore, the abſolute heat which is diſengaged from the air in reſpiration, were not abſorbed by the blood, a very great degree of ſenſible heat would be produced in the lungs.

Again, it has been proved, that the ſame heat which raiſes venous blood 115 degrees, will raiſe arterial only 100 degrees; and, conſequently, the ſame heat, which raiſes venous blood any given number of degrees, will raiſe arterial, a leſs number, in the proportion of 100 to 115, or of 20 to 23; but we know that venous blood contains at leaſt 1580 degrees of heat. Hence, if a certain quantity of venous blood, not in contact with any body that would immediately ſupply it with heat, ſhould ſuddenly be converted into arterial, the heat which was contained in the former would raiſe the latter only $\frac{20}{23}$ of 1580 degrees, or 1373 degrees;

and conſequently, the ſenſible heat would ſuffer a diminution equal to the difference between 1580 and 1373, or nearly 200 degrees. But the common temperature of blood is 96; when, therefore, venous blood is converted into arterial in the lungs, if it were not ſupplied by the air, with a quantity of heat proportionable to the change which it undergoes, its ſenſible heat would be diminiſhed 200 degrees, or it would fall from 96 to 104 below the zero of Farhenheit's ſcale.

That animal heat depends upon the ſeparation of elementary fire from the air in the lungs, is moreover ſupported by the experiments which have been brought in proof of the third and fourth Propoſitions.

For from the experiments which have been adduced in proof of the third Propoſition, it appears, that when bodies are united with the inflammable principle, they part with a portion of their elementary fire; and that when this principle is again diſengaged, they re-abſorb an equal portion of fire from the ſurrounding bodies. It moreover appears, from the experiments which have been adduced in ſupport of the fourth Propoſition, compared with the diſcoveries of Dr. Prieſtley and Mr. Cavendiſh, that when pure air is combined with the inflammable principle, in the ſtate of light inflammable air, it is changed into aqueous vapour; that when it is combined with this principle in the ſtate of heavy inflammable air, it is converted into fixed air; that wax, oil, and tallow conſiſt chiefly of heavy and of light inflammable air in a ſolid ſtate; that when pure air is altered by the combuſtion of theſe ſubſtances, fixed air and aqueous vapour are produced, and that the ſame products are obtained when that fluid is altered by the reſpiration of an animal. Hence it clearly follows, that, in the reſpiration of animals, as well as in the combuſtion of wax, oil, and tallow, the pure air is altered in its properties by its combination with the inflammable principle; and ſince we know that the union of theſe elements is univerſally accompanied with the extrication of heat, and particularly that, by this union, a large quantity of elementary fire is diſengaged from the air in the combuſtion of oleaginous ſubſtances, we may conclude that, in the proceſs of reſpiration, a ſimilar extrication of fire takes place. In this concluſion we are confirmed by the reſults of Experiments VIII. IX. X. and XI. Prop. IV. from which it appears, that when equal quantities of air are changed by the reſpiration of an animal, and by the combuſtion of a wax taper, the quantities of ſenſible heat produced are nearly equal.

Again, it is proved by Experiments I. and II. Prop. II. p. 273, compared with the obſervations in page 84, that the change which the blood undergoes, during its circulation through the lungs, is ſimilar to that which ſolid bodies undergo when they are melted: that is, the capacities are in both inſtances increaſed; and ſince we know that, when ſolids are melted, a quantity of heat is abſorbed, we may conclude that when venous blood is changed into arterial, a ſimilar abſorption takes place.

In the laſt place, it is evinced by Experiments I. II. III. IV. V. VI. and VII. Prop. IV. that when an animal is placed in a cold medium, the venous blood acquires a deeper hue, and that a greater quantity of air is altered by the proceſs of reſpiration in a given time, than when it is placed in a warm medium. Now it has been proved by the very ingenious experiments of Dr. Prieſtley, that the livid colour of the blood in the veins, depends upon its union with the inflammable principle; from which it follows, that the blood is more deeply impregnated with this principle, that it furniſhes a more copious ſupply of it to the air in the lungs; that more air is phlogiſticated in a given time; and that a greater quantity of elementary fire is abſorbed, in proportion as the animal is ſurrounded by a medium which has a lower temperature.

Hence it appears, that the quantity of heat which is ſeparated from the air, and abſorbed by the blood, is, in all caſes, proportioned to the neceſſity. Since, therefore, it has been proved that elementary fire is abſorbed from the air in the proceſs of reſpiration, and ſince the quantity that is thus abſorbed is not only adequate to the effect which we have been endeavouring to explain, but alſo proportional to it, we may ſafely conclude that it is the true cauſe of animal heat.

From the foregoing experiments and obſervations it follows, that animal heat depends upon a proceſs reſembling a chemical elective attraction, The pure air is received into the lungs containing a great quantity of elementary fire; the blood is returned from the extremities impregnated with the inflammable principle; the attraction of pure air to the latter principle is greater than that of the blood. This principle will therefore leave the blood to combine with the air; by this combination the air is obliged to depoſit a part of its elementary fire; and as the capacity of the blood is at the ſame moment increaſed, it will inſtantly abſorb that portion of fire which had been detached from the air. The arterial blood, in its paſſage through the capillary veſſels, is again impregnated with the inflammable principle, in conſequence of which its capacity for heat is diminiſhed, as appears from the experiments which have been recited in proof of the ſecond Propoſition. It will, therefore, in the courſe of the circulation, gradually give out the heat which it had received in the lungs, and diffuſe it over the whole ſyſtem. Thus it appears that, in reſpiration, the blood is continually diſcharging the inflammable principle, and abſorbing heat; and that in the courſe of the circulation, it is continually imbibing this principle, and emitting heat.

Though I have endeavoured to ſhow that the proceſs by which heat is ſeparated from the air, and abſorbed by the blood in the lungs of animals, is analogous to a chemical elective attraction, yet I do not mean to aſſert that elementary fire is really capable of being chemically combined with bodies; much leſs do I affirm, as ſome have ſuppoſed, that it is by means of a double elective attraction, that heat is diſengaged from the air in the proceſs of reſpiration. Before this can be admitted, it muſt be proved that heat is a ſubſtance; and I do not know that any experiments have hitherto been publiſhed which demonſtrate the materiality of that principle. Indeed this opinion is not at all eſſential to the truth of the concluſions contained in the preceding pages; for theſe concluſions have been deduced from facts eſtabliſhed by experiment, and which I think muſt be admitted, whatever be the hypotheſis which we adopt concerning the nature of heat.

Thus, whether we conſider the ſenſible heat of bodies as depending upon a peculiar fluid, or whether we conſider it as an effect produced by a force inherent in bodies, which has by ſome been called the igneous force, or whether we conſider *cold* as ariſing from the action of an inherent force, and *heat* as depending upon the diminution of this force; ſtill, the general properties of heat recited in the foregoing pages muſt, I think, be admitted, becauſe they are deduced from experiment, and from the teſtimony of ſenſe. It muſt, for example, be granted, that the quantity of the element of fire, or of the igneous force in bodies may be increaſed or diminiſhed by their communication with neighbouring bodies, which have a different temperature; that unequal quantities of fire are required in order to produce the ſame degree of ſenſible heat in equal weights of heterogenous bodies; and that a change of form in the ſame body will produce a variation in the quantity of fire which is neceſſary to raiſe it to a given temperature. It is upon theſe general properties, and not upon any hypotheſis reſpecting the nature of heat, that the explanation which has been now given of animal heat is founded. By means of thoſe properties the comparative quantities of that principle contained in various bodies, and particularly, in reſpirable air, and in the blood of animals, have been eſtimated; and if it be admitted as a certain truth, that a *diminution* is produced in the quantity of heat contained in the air, and an *encreaſe* in that contained in the blood, by the proceſs of reſpiration, it will not, I apprehend, be denied that, in this proceſs, heat is continually paſſing from the air to the blood.

It may be proper to add, that, as the blood by its impregnation with the inflammable principle, has its capacity for heat diminiſhed, ſo, on the contrary, thoſe parts of the ſyſtem, from which it receives this principle, will have their capacity for heat increaſed, and will conſequently abſorb heat.

Now if the changes in the capacities, and the quantities of matter changed in a given time, were ſuch that the whole of the elementary fire ſeparated from the air were abſorbed, it is manifeſt that no part of the heat which is received in the lungs would become ſenſible in the courſe of the circulation.

That this, however, is not the caſe, will I think be evident from the following conſiderations.

We know that ſenſible heat is produced by the circulation of the blood; and we have proved by experiment, that a quantity of elementary fire is communicated to that fluid in the lungs, and is again diſengaged from it in its progreſs through the ſyſtem. If, therefore, the whole of the fire, which is ſeparated from the blood, were abſorbed by thoſe parts of the ſyſtem from which it receives the inflammable principle, it would be neceſſary to have recourſe to ſome other cauſe, to account for the ſenſible heat which is produced in the circulation. But by the rules of philoſophiſing, we are to admit no more cauſes of natural things, than ſuch as are both true and ſufficient to explain the appearances *.

That a part of the fire which is ſeparated from the blood, in the courſe of the circulation, becomes redundant, is moreover ſupported by a very extenſive analogy. For the proceſs by which the arterial blood is phlogiſticated during its paſſage through the minute veſſels, is ſimilar to that by which pure air is phlogiſticated in the combuſtion of oleaginous ſubſtances. In the latter proceſs, the inflammable principle is ſeparated from an earthy baſis, and combined with the air;

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* Vide Newtoni Princip. lib. iii. p. 202.

in the former, it is ſeparated from the putreſcent parts of the ſyſtem, and combined with the blood. In both caſes, the capacity of the body which parts with the inflammable principle is increaſed, and that of the body which receives it is diminiſhed; and ſince in the combuſtion of oleaginous ſubſtances, the changes which take place in the capacity of the body which receives this principle, and of that which parts with it, bear ſuch a proportion to each other, that a quantity of heat becomes redundant, we may conclude, that in the circulation of the blood through the minute veſſels, a ſimilar effect is produced.

It muſt nevertheleſs be granted, that thoſe parts of the ſyſtem which communicate the inflammable principle to the blood, will have their capacity for containing heat increaſed; and, therefore, that a part of the fire which is ſeparated from the blood will be abſorbed.

To this cauſe it is probably owing, that leſs heat is produced by an animal, than by the burning of a wax taper, the quantities of air conſumed being equal; for it is likely, that the putreſcent juices, as ſoon as they have communicated the inflammable principle to the blood, fly off in the form of inſenſible

ſenſible perſpiration, and carry along with them a part of the fire which is detached in the courſe of the circulation.

From the quantity of heat, however, which becomes ſenſible in the animal body, it is manifeſt, that the portion of fire which is thus abſorbed, is not very conſiderable.

It appears, therefore, that the blood, in its progreſs through the ſyſtem, gives out the elementary fire which it had received from the air in the lungs; a ſmall portion of this fire is abſorbed by thoſe particles which impart the inflammable principle to the blood; the reſt becomes redundant, or is converted into moving and ſenſible heat.

Of the Inflammation of combuſtible Bodies.

From the above experiments we learn, that atmoſpherical air contains much elementary fire or abſolute heat; that when it is converted into fixed air and aqueous vapour, the greater part of this heat is detached; that the capacities of bodies for containing heat are increaſed by the changes which they undergo in the proceſs of combuſtion. Hence we infer, that the heat which is produced by combuſtion, is derived from the air, and not from the inflammable body.

For inflammable bodies contain little abſolute heat; atmoſpherical air, on the contrary, abounds with this principle. In the proceſs of inflammation, the air is converted into fixed air and aqueous vapour, and at the ſame time gives off a very great proportion of its abſolute heat, which, when extricated ſuddenly, burſts forth into flame, and produces an intenſe degree of ſenſible heat. We have found by calculation, that the heat which is produced by the converſion of atmoſpherical into fixed air, is ſuch, if it were not diſſipated, as would be ſufficient to raiſe the air, ſo changed, to four times the exceſs of the heat of red hot iron above the common temperature of the atmoſphere. It appears, therefore, that, in the proceſs of inflammation, a very great quantity of heat is derived from the air.

It is manifeſt, on the contrary, that no part of the heat can be derived from the combuſtible body; for the combuſtible body, during the inflammation, undergoes a change ſimilar to that which is produced in the blood by the proceſs of reſpiration, in conſequence of which its capacity for containing heat is increaſed; it therefore will not give off any part of its abſolute heat, but like the blood in its paſſage through the lungs, it will abſorb heat.

The calx of iron, for example, is found to contain more than twice as much abſolute heat, as the iron in its metallic form. From which it follows that, in the proceſs of inflammation, the former muſt neceſſarily abſorb a quantity of heat, equal to the exceſs of its heat above that of the latter.

Now whence does it receive this heat? It cannot receive it from the iron; for the quantity of heat in the calx is more than double of that which was contained in the iron, previouſly to the calcination.

But in the inflammation of iron, the atmoſpherical air, with which it is in contact, is altered in its properties, a very great proportion of its abſolute heat is ſeparated, part of which is abſorbed by the calx, and the reſt appears in the form of flame, or becomes moving and ſenſible heat.

We may conclude, therefore, that the ſenſible heat, which is excited in combuſtion

tion, depends upon the ſeparation of abſolute heat from the air.

In confirmation of this concluſion, it may be proper to add, that (if we except the change that the air undergoes in the proceſs of reſpiration, in which the heat is abſorbed,) the ſudden converſion of the purer part of atmoſpherical into fixed air, or into aqueous vapour, is invariably accompanied with the production of ſenſible heat. Thus ſenſible heat is produced when pure air is exploded with inflammable air, when it is diminiſhed and rendered noxious by putrefaction, by the combuſtion of oleaginous ſubſtances, and by the electric ſpark. If the quantity of air, which is changed by theſe proceſſes, in a given time, be very great, the change is attended with much light, with a vivid flame, and with intenſe heat; but if the alteration in the air be ſlow and gradual, the heat paſſes off imperceptibly to to the ſurrounding bodies.

It appears upon the whole, that atmoſpherical air contains in its compoſition a great quantity of fire, or of abſolute heat. By the ſeparation of a portion of this fire in the lungs, it ſupports the temperature of the arterial blood, and thus communicates that *pabulum vitæ*, which is ſo eſſential to the preſervation of the animal kingdom; and, finally, by a ſimilar proceſs, it maintains thoſe natural and artificial fires which are excited by the inflammation of combuſtible bodies.

It has been ſuppoſed by ſome Philoſophers, and particularly by Mr. De Luc and Mr. Lavoiſier, that the heat which ariſes from the inflammation of combuſtible bodies, does not depend upon a change of capacity in the air, but upon a chemical decompoſition.

Theſe Philoſophers take it for granted that elementary fire is a ſubſtance. They divide it into two kinds, fixed and free fire. The latter is that portion of the element of fire which is known to exiſt, in all parts of nature, in a moving and ſenſible ſtate, and which is continually diſpoſed to flow from warmer into colder bodies; and fixed fire is a portion of the ſame element, which the various claſſes of natural bodies are conceived to retain in a ſtate of chemical union, according to their ſeveral attractions. The fire which is thus chemically combined with bodies, is ſuppoſed to loſe its diſtinguiſhing properties. Agreeably to this it is conceived that, during the combuſtion of inflammable and dephlogiſticated air, the fire which was chemically united to theſe ſubſtances, and was retained by them in a fixed and inſenſible ſtate, is, in conſequence of their union, extricated by a ſuperior attraction, when it acquires the form of free and moveable fire.

Whether the production of heat, in this proceſs, ariſes from a change of capacity, or from a chemical decompoſition, can only be determined by an appeal to facts.

I do not know that any experiment has hitherto been made, which ſhows that elementary fire can be chemically combined with bodies, in ſuch a manner as to loſe its diſtinguiſhing properties. In order to eſtabliſh this as a fact, it would be neceſſary to prove that, when bodies produce ſenſible heat by a change of form, their capacities are either undiminſhed, or that the diminutions of capacity, are not in proportion to the quantities of heat produced. Meſſrs. Lavoiſier and De la Place have endeavoured to ſhow that, in ſome inſtances, the quantities of heat evolved, and the changes of capacity, do not correſpond with each other; but they admit that the apparent deviations from this law, were not greater than ſuch as might have ariſen from unavoidable errors in their experiments. I have made many experiments with a view to determine this queſtion, and I have uniformly found that, when bodies produced heat in conſequence of a change of form, their capacities were diminiſhed, and, on the contrary, that when they produced cold, their capacities were increaſed. But I have not yet been enabled to bring my experiments to ſuch a degree of accuracy, as to determine, with certainty, whether the increaſe and diminution of capacity were preciſely proportional to the quantities of heat and cold produced. The reſults, however, were by no means inconſiſtant with that law, and did not deviate from it more than might naturally be expected, from the inaccuracies to which ſuch experiments are liable. It is poſſible, as has been ſuggeſted by Mr. De Luc, that when a body ſuffers an increaſe of capacity, a quantity of elementary fire may be chemically combined with it, and that when the capacity of the body is again diminiſhed, the heat which is evolved may partly depend upon the diminution of its capacity, and partly upon a chemical decompoſition.

In order to determine whether any part of the heat which is abſorbed by bodies, when their capacities are increaſed, enters into a chemical union with them, a ſeries of accurate experiments ſhould be inſtituted, with a view to diſcover the loweſt degree of heat, according to the method which was firſt propoſed by Dr. Irvine, and which has been briefly ſtated to the public by Mr. Magellan, and Meſſrs. Lavoiſier and De la Place.

If it be found that the loweſt degree of heat, as determined by theſe experiments, is conſtant, it will follow that heat does not, in any caſe, enter into a chemical union with bodies; and, conſequently, that the ſenſible heat and cold which they produce, when they undergo a change of form, ariſe ſolely from an alteration of capacity; but if the loweſt degree of heat be not conſtant, we may infer that a part of the heat, which is abſorbed by bodies, when their capacities are increaſed, enters into a chemical union with them, and loſes its diſtinguiſhing properties.

This will be evident if we conſider, that the determination of the loweſt degree of heat, as calculated according to Dr. Irvin's rule, depends upon the relation which the diminution of the capacity bears to the increaſe of the ſenſible heat, or the reverſe. Thus, in the foregoing experiments with pure and inflammable air, the diminution of capacity was 6.07, and the increaſe of the ſenſible heat was nearly 1300 degrees, whence the loweſt degree of heat was found to be 1500 nearly; but if the increaſe of the ſenſible heat had been 2600 degrees, the diminution of capacity remaining the ſame, the loweſt degree would have been nearly 3000. Hence it is manifeſt that, if in *ſome* bodies, the augmentations of temperature, which are produced by an alteration of form, ariſe partly from a diminution of capacity, and partly from the extrication of a portion of heat by a chemical decompoſition, and if, in *other* bodies, the augmentations of temperature ariſe ſolely from a diminution of capacity; the zero or loweſt degree of heat, as calculated from the alterations of temperature and of capacity in theſe bodies, will vary: for, in the former inſtance, it will appear to be farther removed from the common temperature of the atmoſphere, than in the latter. It is, therefore, evident that the queſtion reſpecting the chemical combination of fire will be reſolved, if a ſeries of accurate experiments be inſtituted, in order to diſcover the changes of capacity, which ariſe from alterations of form in different bodies, together with the quantities of heat and cold produced by thoſe alterations *. In the mean time we may obſerve that the extrication of heat from bodies, when they paſs from the ſtate of air or of vapour into a liquid, or a ſolid ſtate, and the abſorption of it when they undergo a contrary change, have been proved by experiment, to depend partly upon an alteration of capacity. It is, therefore, more agreeable to the ſimplicity of nature to conclude that the phænomena ariſe ſolely from that cauſe.

In the courſe of the foregoing experiments it has been ſhown, that given quantities of fire, or of abſolute heat, produce unequal alterations of temperature in equal weights of heterogeneous bodies; and that the quan-

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* The reader will find this ſubject more fully diſcuſſed in the appendix.

tities of heat required to produce a given alteration of temperature in the ſame body, while its form remains unchanged, is nearly uniform; from which it follows, that the element of fire muſt be variouſly diſtributed among the different claſſes of natural bodies, each body requiring more fire or abſolute heat, to raiſe it from the point of total privation to the common temperature of the atmoſphere, in proportion as its temperature is leſs augmented by the addition of a given quantity of that principle.

Dr. Irvine has proved, that when bodies undergo a change of form, their capacities for heat are, in various inſtances, increaſed or diminiſhed: in other words, when the form of a body is changed, a greater or leſs alteration will be produced in its temperature, by a given quantity of fire or of abſolute heat, than would have been produced, if the ſame quantity of fire had been applied to it, previouſly to the change of form; and hence an alteration in the forms of bodies muſt neceſſarily be accompanied with an extrication or abſorption of heat.

From the foregoing experiments it appears that, by the union of dephlogiſticated and nd inflammable air, the capacities of theſe luids are greatly diminiſhed. Whence it ecessarily follows, that this proceſs muſt be ccompanied with the production of much enſible heat. For ſince the fluids have their apacities diminiſhed, at the moment of their nion, a greater increaſe will be produced in heir temperature, by the application of a iven quantity of fire, than would have been produced, if the ſame quantity of fire had been applied to them previouſly to their union; and conſequently the ſame quantity of fire which, previouſly to their union, was ſufficient to raiſe them, from the point of total privation, to the common temperature of the atmoſphere, will now be ſufficient to raiſe them to a much higher temperature.

In like manner as if a cylindrical veſſel, containing water, have its width ſuddenly diminiſhed, the water will neceſſarily riſe to a greater height in the veſſel.

In attempting, therefore, to account for the heat produced by the union of inflammable and dephlogiſticated air, we have diſcovered a cauſe which is proved by experiment to exiſt, and which is found to operate in all other ſimilar inſtances, as far as our obſervations extend. The heat produced by the proceſs in queſtion is, conſequently, reſolved into a general law of nature; and this law ſeems to afford an adequate explanation of the phænomena.

Since, therefore, the exiſtence of a cauſe, ſufficient to account for the appearances, has been eſtabliſhed by experiment, it ſeems inconſiſtent with the ſimplicity of the operations of nature, to ſuppoſe that other cauſes, for the exiſtence of which there is no experimental proof, concur in the production of the ſame effects; for nature does nothing in vain, *& fruſtra fit per plura quod fieri poteſt per pauciora* *.

Hence we may conclude, in general, that the evolution of heat from the air, in conſequence of an alteration in its capacity, is the true cauſe of animal heat, and of that which is produced by the inflammation of combuſtible bodies.

I ſhall next endeavour to ſhow that this doctrine affords an eaſy ſolution of the moſt remarkable facts, relating to animal heat and combuſtion.

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* Vide Newtoni Princip. lib. iii. p. 357.

SECTION IV.

Of the principal Facts relating to Animal Heat.

1. The above doctrine explains the reaſon why the breathing animals have a higher temperature than thoſe who are not furniſhed with reſpiratory organs; for it has been proved that the former are continually abſorbing heat from the air: And, it is probable, that, to provide an apparatus for the abſorption of heat, was the chief purpoſe of nature, in giving to ſo great a part of the animal creation, a pulmonary ſyſtem, and a double circulation.

We have ſhown that animal heat, and the inflammation of combuſtible bodies, depend upon the ſame cauſe, that is, upon the ſeparation of abſolute heat from the air, by a change in its properties.

The quantity of air changed by a man in a minute, is found, by experiment, to be equal to that which is altered by a candle, in the ſame ſpace of time. And hence a man is continually deriving as much heat from the air, as is produced by the burning of a candle.

It is remarked by naturaliſts, that the cold animals have alſo the power of keeping themſelves at a temperature, ſomewhat higher than the ſurrounding medium. To account for this, we may obſerve, that animal heat depends, indirectly, upon a change produced in the air by reſpiration, and directly upon a change which the blood undergoes in the courſe of the circulation.

The blood during its paſſage through the capillary veſſels, has its capacity for heat diminiſhed, and by this diminution is obliged to give out a part of its abſolute heat. The ſource from which it is again ſupplied with heat, in ſuch of the cold animals as are not furniſhed with lungs, can only be determined by experiment. It is probable, that, in thoſe animals, the aliment contains more abſolute heat than the blood. If this be the caſe, the blood will be ſupplied with heat from the aliment.

2. From the intereſting and deciſive experiments which were made in heated rooms, by Dr. Fordyce, it appears, that the animal body has, in certain ſituations, the power

of producing cold, or of keeping itſelf at a lower temperature than the ſurrounding medium.

This power has been attributed by ſome philoſophers to the evaporation from the ſurface of the body; and indeed it muſt be allowed, that the increaſed evaporation will have a very conſiderable influence in diminiſhing the heat; but the experiments of Dr. Fordyce prove that the phænomenon does not ariſe ſolely from that cauſe. For the doctor, remained in moiſt air, heated to 130 during the ſpace of 15 minutes, when the thermometer under his tongue ſtood at 100; and ſtreams of moiſture ran down over his whole body, proceeding from the condenſation of vapour, as evidently appeared from a ſimilar condenſation on the ſurface of a Florentine flaſk that had been filled with water at 100.

He, moreover, found, in the courſe of his experiments, that a dog could live without much inconvenience for a conſiderable time in air heated to 260, the body of the animal preſerving very nearly its proper temperature, being never raiſed more than two degrees above the natural ſtandard.

From the former of theſe experiments it appears, that the animal body poſſeſſes the power of producing cold, in a moiſt as well as in a dry air; and it is manifeſt that, in the latter experiment, the refrigeration was much greater than that which would have been produced by an equal maſs of inanimate matter, containing an equal quantity of moiſture. Whence it follows that, in living animals, the faculty of generating cold does not ariſe ſolely from evaporation.

This concluſion is moreover confirmed by the experiments which follow:

A living and dead frog equally moiſt, and of nearly the ſame bulk, the former of which was at 67, the latter at 68, were laid upon flannel in air which had been raiſed to 106.

Two ſenſible thermometers were placed in contact with the ſkin of the animals under the axillæ, and the order of cooling was obſerved during 25 minutes.

Minute.	Air.	Dead Frog.	Living Frog.
In 1	——	70½	67½
2	102	72	68
3	100	72½	69½
4	100	72½	69½
25	95	81¼	78¼

The thermometers being introduced into the ſtomach, the internal heat was found to be the ſame with that at the ſurface.

Hence it appears, that the living frog acquired heat more ſlowly than the dead one. Its vital powers muſt, therefore, have been active in the generation of cold.

To diſcover whether the cold produced in this inſtance, depended ſolely upon the evaporation from the ſurface increaſed by the energy of the vital principle, a living and dead frog were taken at 75, and were immerſed in water at 93, the living frog being placed in ſuch a ſituation as not to interrupt reſpiration.

Minute.	Dead Frog.	Living Frog.
In 1	85	81
2	88½	85
3	90½	87
5	91½	89
6	91½	89
8	91½	89

Since, therefore, living frogs have the faculty of producing cold in warm water, as well as in warm air; and ſince the human body has the ſame power in a moiſt as well as in a dry air, we may conclude that this power does not depend ſolely upon evaporation.

I ſhall now endeavour, from the preceding facts, to explain what appear to me to be the true cauſes of the cold produced by animals, when placed in a medium, the temperature of which is above the ſtandard of their natural heat.

From the experiments on venous and arterial blood, and from the calculation in page 357, it appears, that the capacity of the blood for containing heat is ſo much increaſed in the lungs, that, if its temperature were not ſupported by the heat which is ſeparated from the air in the proceſs of reſpiration, or from the parts adjacent, it would ſink 200 degrees. Hence, if the evaporation from the lungs be ſo much augmented as to carry off the whole of the heat that is detached from the air, the arterial blood, when it returns by the pulmonary vein, will have its ſenſible heat greatly diminiſhed, and will conſequently abſorb heat from the veſſels that are in contact with it, and from the neighbouring parts. The heat which is thus abſorbed in the greater veſſels will again be extricated in the capillaries, where the blood receives a freſh addition of the inflammable principle. If, in theſe circumſtances, the blood during each revolution were to be equally impregnated with this latter principle, it is manifeſt that the whole effect of the above proceſs would be to cool the ſyſtem at the centre, and to convey the heat to that part of the body where it is capable of being inſtantly carried off by evaporation.

But it appears from Experiment VII. Prop. V. that when an animal is placed in a heated medium, the ſanguineous maſs during each revolution is leſs impregnated with the inflammable principle; for the venous blood, in theſe circumſtances, becomes gradually paler and paler in its colour, till at length it acquires nearly the appearance of the arterial; and it is proved, by the experiments of Dr. Prieſtley, that the dark and livid colour of the blood in the veins, depends upon its combination with the inflammable principle in the minute veſſels. Since, therefore, in a heated medium, this fluid does not aſſume the ſame livid hue, we may conclude that it does not attract an equal quantity of the phlogiſtic principle;

and in this concluſion we are confirmed by Experiments I. II. III. IV. V. and VI. Prop. V. which prove that the quantity of air which an animal phlogiſticates, in a given time, when it is placed in a warm medium, is leſs than that which it phlogiſticates in an equal ſpace of time when it is placed in a cold medium.

It follows, from theſe facts, that the quantity of heat given off by the blood in the capillaries, will not be equal to that which it had abſorbed in the greater veſſels, or poſitive cold will be produced. If the blood, for example, in its paſſage to the capillaries, abſorb from the greater veſſels a quantity of heat as 200, and if, in conſequence of its receiving a leſs impregnation of the phlogiſtic principle than formerly, it give off at the extreme veſſels a quantity of heat only as 150, it is manifeſt that upon the whole a degree of refrigeration will be produced as 50: and this cauſe of refrigeration will continue to act, while the colour of the venous blood is gradually approaching to that of the arterial, till its approximation becomes the neareſt poſſible in the circumſtances in which the animal is placed, after which it will ceaſe to operate. Thus it appears that, when animals are placed in a warm medium, the ſame proceſs which formerly ſupplied them with heat, becomes for a time the inſtrument of producing cold, and probably preſerves them from ſuch rapid alterations of temperature as might prove fatal to life.

Upon the whole, the increaſed evaporation from the ſurface, and the diminution of that power by which the blood, in its natural ſtate, is impregnated with the phlogiſtic principle, ſeem to be the great cauſes on which the refrigeration depends.

By the firſt of theſe cauſes the animal is cooled at the ſurface, and by the ſecond an accumulation of heat at the centre is obviated.

The power which animals poſſeſs of cooling themſelves at the centre, when they are placed in a medium above the ſtandard of their natural heat, ſeems to be peculiarly neceſſary to the preſervation of life. For as the heat which is received at the ſurface, in thoſe circumſtances, is ſpeedily carried towards the internal parts by the reflux of the heated fluids; if it were not abſorbed and rendered inſenſible by the change which the blood undergoes in the lungs, the central heat would probably ſoon be increaſed to ſuch a degree as to deſtroy the vital principle.

3. We may hence perceive the reaſon why animals preſerve an equal temperature, notwithſtanding the great variations that take place in the heat of the atmoſphere, ariſing from the viciſſitudes of the weather, and the difference of ſeaſon and climate.

From the experiments which have been recited in proof of the fourth Propoſition, it appears that, in the proceſs of reſpiration, the difference between the purity of the inhaled and expired air is increaſed by expoſing the animal to cold; and likewiſe that the difference between the colour of the arterial and venous blood is augmented from the ſame cauſe. But the quantity of fire, ſeparated from the air, muſt neceſſarily be in proportion to theſe differences. For as the extrication of heat from that element is produced by its phlogiſtication, it is manifeſt that the more compleatly a given portion of air is phlogiſticated, the greater will be the quantity of heat detached from it. The ſame remark will apply to the phlogiſtication of the blood. It follows, that as ſoon as, by expoſure to cold, an unuſual diſſipation of the vital heat is produced, the blood in the courſe of the circulation will begin to be more deeply impregnated with the phlogiſtic principle; it will, therefore, furniſh a more copious ſupply of that principle to the air in the lungs, and will imbibe a greater quantity of fire in return. Add to this, that by the tonic and ſtimulant powers of cold, the veſſels on the ſurface are conſtricted, the inſenſible perſpiration is diminiſhed, the blood is determined to the lungs, the pulſe and reſpiration are rendered full and frequent, and conſequently as the cold advances in winter ſeaſons, and in northern climates, the vigour of the animal powers will be increaſed, and this will be accompanied with a proportional increaſe in the faculty of abſorbing heat from the air.

In ſummer, on the contrary, the reverſe of this will take place. The powers of life will be relaxed; the air will be leſs tainted by reſpiration; a ſmaller portion of the phlogiſtic principle will be attracted by the blood in the minute veſſels; and the quantity of fire which is abſorbed will be proportionally diminiſhed, at the ſame time that the evaporation from the ſurface is increaſed.

It appears, therefore, that the heating and cooling powers in the animal body are adjuſted to each other in ſuch a manner as to produce an equal effect; and, conſequently, the law by which animals maintain an uniform temperature, notwithſtanding the variations in the heat of the ſurrounding air, is ſimilar to that by which the temperatures of bodies become fixed when they have arrived at the melting or freezing points. When freezing water, for example, is expoſed to a cold atmoſphere, the quantity of elementary fire carried off by the ſurrounding air, is preciſely balanced by that which is extricated from the water at the moment of its congelation. In like manner, when a living animal is expoſed to a cold atmoſphere, the quantity of heat carried off by the circumambient medium, is balanced by that which is detached from the blood in the courſe of the circulation. On the contrary, when ice is placed in a warm atmoſphere, the whole of the heat which it receives is rendered inſenſible; and it has been ſhown that, when an animal is placed in a warm medium, a ſimilar extinction of the heat takes place.

Hence the power which animals poſſeſs, of generating heat, is in all caſes proportioned to the demand. It is increaſed by the winter colds, diminiſhed by the ſummer heats; it is totally ſuſpended, or converted into a contrary power, according as the exigencies of the animal may require.

4. Among different animals, thoſe are the hotteſt, which breathe the greateſt quantity of air in proportion to their bulk; and in the ſame animal, the degree of heat is in ſome meaſure proportionable to the quantity of air inhaled in a given time.

Theſe varieties appear to be the neceſſary conſequence of the general fact, that the heat of the breathing animals is derived from the air. For if animal heat depends upon a change which the air undergoes in the lungs, it is evident that, all other circumſtances being equal, the greater the quantity of air which is changed in a given time, the greater muſt be the heat produced.

In exerciſe, by the action of the muſcles, the venous blood is returned in greater quantities than uſual, from the extremities, to the right auricle of the heart. By the action of the heart it is determined to the lungs. The reſpiration is accelerated; the velocity of the circulation is increaſed; and hence, a proportionable increaſe in the quantity of heat abſorbed.

The cold ſtage of fevers is preceded by languor, a ſenſe of debility, and a diminution in the action of the heart and arteries. The reſpiration is ſmall, the pulſe is weaker than natural—the quantity of blood which paſſes through the lungs, in a given time, is diminiſhed—and hence leſs phlogiſton will be diſcharged from the blood, and, conſequently, leſs heat will be ſeparated from the air.

In the progreſs of the cold ſtage, a ſpaſm is formed upon the ſurface *. By the conſtriction of the veſſels on the ſurface, the blood is determined to the heart; the heart is ſtimulated to more frequent and violent contractions; the velocity of the blood through the lungs is increaſed; the reſpiration is accelerated; and hence a greater quantity of heat will be abſorbed.

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* See Cullen's firſt Lines of the Practice of Phyſick.

We may obſerve, that the abſorption of heat, and the accelerated velocity of the blood through the lungs, will act and react upon each other, in ſuch a manner, as that the heat will have a conſtant tendency to increaſe. For the accelerated velocity of the blood occaſions a greater abſorption of heat; and the increaſed abſorption of heat, by ſtimulating the heart and arteries to more frequent and powerful contractions, will again accelerate the velocity of the blood, which will ſtill farther increaſe the abſorption. And, therefore, the heat will continue to be accumulated, till counteracted by the operation of ſome other cauſe. From the ſudden diminution in the weight of the body, notwithſtanding the quantity of watery fluids that are taken in, and the obſtruction of the urinary ſecretion, it appears, that, in the hot ſtage of fevers, there is a very great evaporation from the ſurface; we may, therefore, conclude, that this is one of the means which Nature employs, for moderating the heat, and reſtraining the violence of the diſeaſe.

Another cauſe which prevents the accumulation of heat, is the cooling power of the external air. We have already obſerved, that the quantities of heat loſt by a body, in a given time, are in proportion to the exceſs of its heat, above that of the ſurrounding medium. If, therefore, the ſenſible heat of the body increaſe, while the temperature of the air continues the ſame, the quantities of heat carried off by the latter, in a given time, will be proportionably increaſed.

In putrid fevers, to the accelerated velocity of the blood through the lungs, there is added a putreſcent ſtate of the ſyſtem; in conſequence of which, the air inhaled in inſpiration, will be more copiouſly ſupplied with the inflammable principle, than when the body is in a ſound and healthy ſtate. If, in the latter inſtance, the air which is received into the lungs, were completely ſaturated with this principle, the quantity of heat ſeparated from it, would always be proportionable to the quantity of air inhaled in a given time. But it appears from experiment, that the air which is expired by a healthy animal, is not completely ſaturated with the inflammable principle. It is capable of being farther diminiſhed by nitrous air, and not more than the eighth part of it conſiſts of fixed air. The quantity of heat, therefore, which is ſeparated from the air in reſpiration, will be partly in proportion to the quantity of air inſpired, and partly to the quantity of the inflammable principle diſcharged from the blood, in a given time.

In fevers of the putrid kind, as the ſolid and fluid parts of the ſyſtem are in a putreſcent ſtate, and, conſequently, retain the principle of inflammability with leſs force, a greater quantity of this principle will be diſcharged from the lungs, the air will be more copiouſly ſupplied with it in the proceſs of reſpiration, and will, therefore, impart to the blood a greater proportion of its abſolute heat. To theſe cauſes it is probably owing, that the heat of the human body never riſes ſo high as in putrid fevers.

5. Topical inflammation is accompanied with redneſs, with tumour, and with unuſual heat *. From the throbbing of the veſſels, and from microſcopical obſervations, it appears, that the velocity of the blood through the part inflamed, is accelerated; and it is manifeſt, that a tendency to putre-

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* See Cullen's firſt Lines of the Practice of Phyſick.

faction muſt be produced by the violent reaction, and by the ſtagnation of the ſerous fluid which is ſometimes effuſed into the adjoining cellular texture. It has been already obſerved, that the arterial blood has a ſtrong attraction to phlogiſton, and that by its union with this principle, in the courſe of the circulation, it is obliged to give out that heat which it had received in the lungs. In the ſtate of health, the velocity of the blood through the different parts of the ſyſtem, and the quantities of phlogiſton with which it is ſupplied in thoſe parts, are adjuſted to each other in ſuch a manner, that the heat is equally diffuſed over the whole. But, if by any irregularity, the balance be deſtroyed; if, by the increaſed action of the veſſels, the blood be urged with greater violence than uſual through any particular part, or, in conſequence of a greater tendency to putrefaction, be more copiouſly ſupplied with phlogiſton, it is manifeſt, that a greater quantity of heat will be extricated in that part, in a given time. This heat will ſtimulate the veſſels into more frequent and forcible contractions, by which the velocity of the blood, and the conſequent extrication of heat will be ſtill farther increaſed. On this principle we may probably account for the partial heats which are produced by topical inflammations, and for thoſe which ariſe in hectic and nervous diſeaſes.

It will hereafter appear, that the heat is accumulated in topical inflammation, by the increaſed velocity of the blood through the part inflamed, in the ſame manner as it is accumulated upon the fuel, in combuſtion, by directing a ſtream of freſh air into the fire.

6. I ſhall obſerve, in the laſt place, that the doctrine contained in the preceding pages, reſpecting the cauſe of animal heat, will probably lead to an explanation of the uſes of the ſpleen and of the lymphatic glands.

May not the ſpleen be intended partly to apply a proper degree of warmth to the left ſide of the ſtomach, and partly to ſupply the liver with phlogiſticated blood? As a quantity of fire is extricated during the converſion of arterial into venous blood, in the minute veſſels, if the blood which is phlogiſticated in the ſpleen had paſſed to the liver in the ſtate of arterial blood, and had been changed into venous, in that organ, it ſeems evident that there would have been a redundancy of heat in the right hypochondriac region, and a deficiency of it in the left; and this effect would have been produced in a much greater degree, if the blood which comes from the inteſtines had likewiſe paſſed to the liver in the ſtate of arterial blood. But in the preſent ſituation of things, the blood which flows through the celiac and meſenteric arteries, having communicated the heat which it had abſorbed in the lungs to the inteſtines, and to the left ſide of the ſtomach, paſſes to the liver impregnated with the inflammable principle; in conſequence of which it is not any longer in a condition to give off heat, and at the ſame time, by its union with the principle of inflammability, it is probably brought into that ſtate in which it is beſt fitted for the ſecretion of the bile.

As to the lymphatic glands, there is reaſon to believe that the arterial blood, which is converted into venous, in thoſe glands, attracts from the lymph a quantity of the inflammable principle, and communicates to it a portion of elementary fire. It is probable that the lymph, by this alteration, is rendered more fit for becoming one of the conſtituent parts of the blood.

Of the principal Facts relating to the Inflammation of Combuſtible Bodies.

1. We have proved, that the ſenſible heat in combuſtion is derived from the air, and depends upon the ſeparation of abſolute heat from that element. Hence it is evident, that when the air, in which an inflamed body is confined, is deprived of the greater part of its abſolute heat, the ſource of inflammation will be exhauſted, and the flame will neceſſarily be extinguiſhed. And this explains the reaſon, why a conſtant ſucceſſion of freſh air is neceſſary to inflammation, as well as to the ſupport of animal life.

2. As the ſenſible heat, which is produced by combuſtion, depends upon the ſeparation of abſolute heat from pure air, it is manifeſt that the greater quantity of pure air any ſpecies of aërial fluid contains, the longer it muſt contribute to the ſupport of flame, as well as to the preſervation of animal life. Agreeably to this we find that the quantity of pure air, contained in the

beſt dephlogiſticated air, is nearly four times as great as in common air; and Dr. Prieſtley has ſhown that five times as much ſenſible heat is produced by the converſion of it into fixed air, as by that of common air; for a candle will continue to burn five times as long in the former as in the latter ſpecies of air*.

3. As the ſenſible heat, in combuſtion, depends upon a change produced in the air, it is manifeſt, that (all other circumſtances being equal) the intenſity of the heat muſt be in proportion to the quantity of air which is changed in a given time. And hence the heat may be increaſed to a very great degree, if a ſtream of freſh air be directed upon the fuel by bellows, or by the blow pipe.

We have found, that by the converſion of dephlogiſticated into fixed air, a quantity of heat is diſengaged, which would be ſufficient to raiſe the air ſo converted to more than four times the exceſs of the heat of red hot iron above the common temperature of the atmoſphere. And indeed the degree of heat, excited by the inflammation of combuſtible bodies, would not be leſs than this, if the fire that is thus extricated, were applied to the fixed air alone, and were to remain in the ſame concentrated ſtate, in which it is at firſt ſeparated from the atmoſpherical air.

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* In phlogiſtic proceſſes, a ſmall portion of the pure air contained in atmoſpherical, is protected from the action of the inflammable principle, by the phlogiſticated air which forms one of the conſtituent parts of the atmoſphere. See p. 272.

But as ſenſible heat has a conſtant tendency to an equal diffuſion, it will inſtantly flow from the point inflamed, and ſpread itſelf over the ſurrounding bodies. It will be accumulated upon the fuel, abſorbed by the vapour, and communicated to the atmoſphere. This explains the reaſon, why the heat is ſo intenſe in the flame of a candle, and is ſo greatly diminiſhed at the ſmalleſt diſtance from the flame.

4. Though it is highly probable that all inflammable bodies have their capacities for containing heat changed by the alterations which they undergo, in the proceſſes of calcination and combuſtion, yet from the experiments which have been recited above, it is manifeſt, that the degree of this change is very different in different bodies. We have ſeen that the capacity of the calx of iron is

to that of iron, as 1.9 to 1; that the capacity of the calx of antimony is to that of the regulus as 3.5 to 1; that the capacity of pure air is to that of fixed air as 4 to 1; and that the capacity of arterial blood is to that of venous as 11½ to 10.

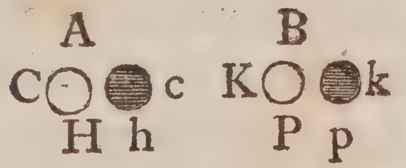
It appears, moreover, that different quantities of the inflammable principle are required to ſaturate different bodies. Some of the oleaginous ſubſtances abound more with this principle than others; and a greater quantity of it is required to ſaturate pure air than an equal weight of arterial blood.

From theſe facts it follows, that when the inflammable principle paſſes from one body to another, the changes in the capacities of the bodies for containing heat will be different, and unequal quantities of matter will be changed in a given time. Thus, in reſpiration, the inflammable principle is ſeparated from the blood and combined with the air. The diminution which is produced, by this proceſs, in the capacity of the air for containing heat, is greater than the increaſe in that of the blood; and as more inflammable air is required to ſaturate pure air than an equal weight of arterial blood, the quantity of the latter, which is changed in a given time, will be greater than that of the former.

When two contiguous bodies, at the ſame moment, have their capacities for containing heat reſpectively increaſed and diminiſhed, if the changes be ſuch that the whole heat ſeparated from the one is abſorbed by the other, no ſenſible heat or cold will be produced.

The caſes in which this will happen may be determined in the following manner.

PROPOSITION I.



LET there be two bodies, A and B, which have their capacities for containing heat changed at the ſame moment, the capacity of A being diminiſhed, and that of B increaſed; let the capacity of A before the change, be denoted by C, and after the change, by c; the capacity of B before the change by k, and after the change by K. Then C—c will be the difference of the capacities of A before and after the change; and K—k the difference of the capacities of B. It is affirmed, that, if the temperatures and the quantities of matter changed in a given time, be equal, the differences of the capacities will be as the differences of the abſolute heats.

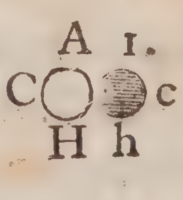
For the capacities of bodies for receiving heat, are eſtimated (as was obſerved above) by the comparative quantities of abſolute heat which they are found to contain, when the quantities of matter are equal, and the temperatures are the ſame. The temperature and the quantities of matter, therefore, in A and B being equal, the capacities will be directly as the abſolute heats. If the abſolute heat of A be double that of B, the capacity will be double, if triple, triple, &c. And therefore, calling the abſolute heats of A and B before the change, H and p, and after the change h and P, it will be, C : c : : H : h. And by converſion C—c : c : : H—h : h. For the ſame reaſon K—k : k : : P—p : p. But becauſe the quantities of matter in A and B are equal, and the bodies before and after the change are conceived to be brought to the ſame temperature, it will be, c : k : : h : p.

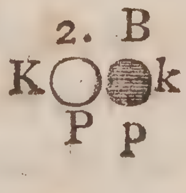
Since therefore C—c : c : k : K—k
as H—h : h : p : P—p, by
equality C—c : K—k : : H—h : P—p

That is, the differences of the capacities are as the differences of the abſolute heats, the temperatures and the quantities of matter being equal.

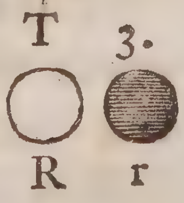
PROPOSITION II.

IF the differences of the capacities be equal, the differences of the abſolute heats will be as the quantities of matter *.


1.


2.

The ſame things remaining as above, if the quantities of matter in A and B be equal, C—c : K—k : : H—h : P—p.


3.

Let the quantity of matter in A be increaſed, in any proportion, as in Fig. 3, and, after the increaſe, let it be called T. Let the abſolute heats be denoted by R and r, and the

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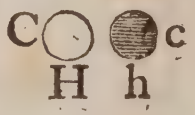
* In this and the following propoſition, the bodies are ſuppoſed to be brought to the ſame common temperature before and after the change.

quantities of matter in A (or B) and T, by q and Q reſpectively.

Since the capacities of A and T are equal, the abſolute heats before the change, will be as the quantities of matter. Therefore H : R : : q : Q. For the ſame reaſon the abſolute heats, after the change, will be as the quantities of matter. That is, h : r : : q : Q. Therefore, H : R : : h : r, and H : h : : R : r, and H—h : h : : R—r : r, and H—h : R—r : : h : r. But h : r : : q : Q. Therefore H—h : R—r : : q : Q. But H—h is equal to P—p. Therefore P—p : R—r : : q : Q. And hence the differences of the capacities being equal, the differences of the abſolute heats will be as the quantities of matter.

PROPOSITION III.

IF the differences of the abſolute heats be equal, the differences of the capacities will be reciprocally as the quantities of matter.



For, by the firſt propoſition, the quantities of matter being given, the differences of the abſolute heats are directly as the differences of the capacities; and, by the ſecond propoſition, the differences of the capacities being given, the differences of the abſolute heats are as the quantities of matter; it follows, that neither being given, the differences of the abſolute heats, are as the differences of the capacities multiplied into the quantities of matter. That is, H—h is as C—c × Q. Therefore C—c will be as $\frac{H-h}{Q}$; and if H—h be given, C—c will be reciprocally as Q. Conſequently if the differences of the abſolute heats be equal, the differences of the capacities will be reciprocally as the quantities of matter.

COR. I. It is, therefore, required, in order that neither heat nor cold ſhould be produced, that the differences of the capacities ſhould be reciprocally as the quantities of matter changed in a given time. For in that caſe, by the converſe of this propoſition, the differences of the abſolute heats will be equal.

Cor. II. See the fig. Prop. I. If, therefore, the diminution in the capacity of A, be to the increaſe in that of B, in a greater proportion, than the quantity of matter in B to that in A, the whole of the heat which is ſeparated from A will not be abſorbed by B, a part of it will become redundant, or will be converted into moving and ſenſible heat.

Cor. III. The differences of the capacities being reciprocally as the quantities of matter, if the quantities of matter changed in a given time be equal, the differences of the capacities will be equal.

Cor. IV. See the fig. Prop. I. The quantities of matter changed being ſuppoſed to be equal, ſince it is required, in order that neither heat nor cold ſhould be produced, that $H-h$ ſhould be equal to $P-p$, it follows that P muſt be equal to $H+p-h$.

If h be equal to p, that is, if $p-h=0$ then $P=H$. If h be greater than p, $p-h$ will be a negative quantity; on the contrary, if h be leſs than p, $p-h$ will be poſitive. In the former caſe, P will be leſs than H, and, in the latter, it will be greater than it.

This may be otherwiſe briefly expreſſed as follows: the quantities of matter changed being equal, let the capacity of A, after the change, and that of B previouſly to it, be called the *leaſt* capacities of theſe ſubſtances, and let the capacity of the former previouſly to the change, and that of the latter ſubſequent to it, be called their *greateſt* capacities.

Since the diminution in the capacity of A is equal to the increaſe in that of B, it is manifeſt, that if the *leaſt* capacities be equal, the *greateſt* capacities will likewiſe be equal. But if the *leaſt* capacities be unequal, this inequality muſt be balanced by a correſponding inequality in the *greateſt* capacities; or in as much as the leaſt capacity of A is greater or leſs than that of B, in ſo much muſt the greateſt capacity of the former be greater or leſs than that of the latter; the difference of the leaſt capacities being, in all caſes, equal to the difference of the greateſt.

The rule which is expreſſed in the firſt corollary, applies to the ſeparation of heat from the air, and the abſorption of it by the blood, in the proceſs of reſpiration. For as the ſenſible heat in the lungs, is not greater than in the other parts of the body, it is manifeſt, that the whole of the heat which is ſeparated from the air muſt be abſorbed. And therefore the changes produced in the blood and the air, are ſuch, that the difference of the capacities of venous and arterial blood, is to the difference of the capacities of fixed and atmoſpherical air, as the quantity of air changed in a given time, is to that of blood: in which caſe, by the above corollary, no part of the heat will become redundant. The oppoſite changes, therefore, which the air and the blood undergo in the lungs, preciſely balance each other. For as the quantity of blood, which is altered by reſpiration, in a given time, is much greater than that of air, ſo the change which is produced in the air, during this proceſs, is proportionably greater than that which is produced in the blood.

The rule which is expreſſed in the ſecond corollary, applies to the ſenſible heat produced in the courſe of the circulation, and to that which is produced by the inflammation of combuſtible bodies. As we find, for example, that a part of the heat which is diſengaged from the blood, in its progreſs through the ſyſtem, becomes redundant, we may conclude, that the diminution in the capacity of the blood, is to the increaſe in the capacity of thoſe parts of the body from which it receives the inflammable principle, in a greater proportion, than the quantity of matter in the latter to that in the former; in which caſe, the whole of the heat ſeparated from the blood will not be abſorbed; a part of it will be converted into moving and ſenſible heat.

That this rule is applicable to the inflammation of combuſtible bodies, appears from the following obſervations and experiments.

In the burning of iron, the metal is converted into a calx, and the purer part of the common air which ſupports the inflammation is abſorbed, either in its ſimple ſtate, or combined with phlogiſton. By this proceſs the capacity of the air for containing heat is diminiſhed, and that of the calx is increaſed; and, therefore, from the firſt and third Corollaries, it follows, that if, in the inflammation of iron, equal quantities of air and iron were changed in a given time, and if the difference of the capacities of iron and of its calx, were equal to the difference of the capacities of the air before and after the change which is produced upon it by its union with the metal, the whole heat extricated from the air would be abſorbed by the calx.

The alteration of capacity which the air undergoes by its union with the metal may be determined as follows.

It has been proved that the quantity of abſolute heat in the calx of iron when ſaturated with air, is to that contained in the ſame calx when the air is in a great meaſure ſeparated by nitrous acid and fire, as 6 to 4. We ſhall ſuppoſe the calx, when ſaturated with air, to contain $\frac{1}{3}$ of its weight of that fluid. If we conceive the whole heat in the aerated calx to be divided into ſix equal parts, the abſolute heat of that ſubſtance, after the ſeparation of the air, will be to its heat, previouſly to the ſeparation, as 2.6666 to 6. For if the calx, after the ſeparation of the air, were equal in quantity to the aerated calx, the heat of the former would be to that of the latter as 4 to 6; but as it is only equal to two thirds of the aerated calx, it will be as 2.6666 to 6.

Let the difference between the heat of the aerated calx, and of that ſubſtance when freed from air, be ſuppoſed to give the heat which was contained in the air during its combination with the metal *. It will follow that this heat will be repreſented by the difference between 2.6666 and 6. That is, the heat contained in the air, will be to that contained in the aerated calx, as 3.3334 to 6, the quantity of matter in the air being one third of that in the calx. And, therefore, taking equal quantities of the air and the calx, the heat of the former will be to that of the latter as 3.3334 multiplied by three, or as 10 to 6 very nearly, or as 1.6666 to 1.

It has been already ſhown that the heat of the calx is to that of water as 1 to 4; therefore the heat of the combined air is to that of water as 1 to 2.4; but the heat of pure air is to that of water as 4.75 to 1, therefore the heat of the pure air is to that

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* In this ſuppoſition it is taken for granted that the capacity of the calx is not altered by its combination with the air. It is probable, however, that it is ſomewhat increaſed. If this be admitted, it will follow that the quantity of heat contained in the air is leſs than that which is ſtated above; but this will not affect the calculation of the heat which becomes ſenſible during the combuſtion of iron; becauſe the error in the ſtatement of the heat contained in the combined air will be balanced by a contrary error in the ſtatement of the heat contained in the calx.

of the combined air as 11.1 to 1, or as 36.4 to 3.28, and the heat of the combined air is to that of the iron as 3.28 to 1. The capacity of the calx of iron is to that of iron as 2 to 1 nearly.

Calling, therefore, the capacity of pure air in its elaſtic form 36.4; and that of the combined air 3.28, it appears from Cor. 1. Prop. III. that if the whole heat ſeparated from the air were abſorbed by the calx, the quantity of iron calcined would be to the quantity of air which is abſorbed during the calcination, as the difference of the capacities of pure and combined air, is to the difference of the capacities of the iron and of the calx.

The quantity of air contained in the calx being $\frac{1}{3}$ of the whole, it follows that the quantity of iron which is altered by the proceſs of calcination is to that of air as 2 to 1; and the difference of the capacities of pure and combined air is 36.4—3.28, or 33.12. Hence, if the whole heat ſeparated from the air were abſorbed, it would be as 2 : 1 : : 33.12 to the difference of the capacities of the iron and the calx. That is, the exceſs of the capacity of the calx above that of the iron would be 16.56; and the quantity of heat in the iron being expreſſed by unity, the capacity of the calx would be to that of the iron as 17.56 to 1; but it is as 2 to 1. Hence the heat which becomes redundant during the calcination of iron, is to the original heat of the iron as 15.56 to 1; and it is to that of the calx as 15.56 to 2, or as 7.78 to 1.

It was before ſhown, that bodies, when at the common temperature of the atmoſphere, contain at leaſt 1500 degrees of heat. During the inflammation of iron, therefore, a quantity of heat becomes redundant, which would be ſufficient to raiſe the calx 1500 degrees multiplied by 7.78, or 11670 degrees.

Hence we may account for the ſpark which is produced by the percuſſion of flint and ſteel. A particle of the metal is ſtruck off by the force of the flint. This particle is altered in ſuch a manner as to be rendered capable of abſorbing a portion of air. The air, at the ſame moment, parts with a quantity of elementary fire, which raiſes the particle to a red heat, and thus the ſpark is produced.

It may be difficult to determine the peculiar mode in which the ſtroke of the flint operates upon the particle of the metal, ſo as to render it capable of combining with air. But the fact is certain, for when a quantity of the metal, calcined by this proceſs, is collected, the greater part of it is found to conſiſt of finery cinder, or of æthiops martialis, which is known to contain $\frac{1}{3}$ of its weight of air, the remaining portion being apparently converted into glaſs.

It is manifeſt, from the foregoing calculation, that the air, which is thus combined with the portion of the metal that is ſtruck off, muſt, at the moment of its abſorption, depoſit a quantity of fire, which would be ſufficient to raiſe the calcined particle 11670 degrees.

That the phenomenon derives its origin from this ſource, is confirmed by experiments which were made on that ſubject by Mr. Lane; from which it appears that a ſpark is not produced by the percuſſion of flint and ſteel in a vacuum. A very ſmall quantity of pure air is, however, ſufficient to produce this effect; for Mr. Kirwan has found that a ſpark is excited by the percuſſion of flint and ſteel under common ſpring water.

It appears, upon the whole, that the moſt important facts relating to the inflammation of combuſtible bodies, may readily be explained upon the principle that the heat which is excited in combuſtion is derived from the air.

A part of this heat is abſorbed by the elements into which the combuſtible body is reſolved, and the reſt becomes redundant, or is converted into ſenſible heat; it follows, that the quantity of fire which is extricated will be in proportion to the quantity of air which is altered in a given time; and that the degree of ſenſible heat which is produced will be greater or leſs, in proportion as a leſs or a greater quantity of this fire is abſorbed by the body which ſupports the inflammation.

In the inflammation of alcohol and ſulphur, a very great proportion of the fire which is detached from the air, is imbibed by the aqueous and ſulphureous vapour; and, therefore, alcohol and ſulphur burn with a pale and weak flame. On the other hand, thoſe inflammable bodies which produce little vapour, or which produce a vapour that is capable of abſorbing but little heat, as pit-coal, oil, wax, phoſphorus, burn with a ſtrong and vivid flame; for, in theſe caſes, a great part of the fire which is yielded by the air, is converted into ſenſible heat.

I have thus endeavoured to account for the phenomena of combuſtion and of animal heat, from the general fact that the capacity of pure air for containing heat is diminiſhed by the reſpiration of animals, and by the action of inflammable bodies.

On this principle, a variety of phenomena may be explained, beſides thoſe which have been already mentioned.

It is probable that the vapour of pure nitrous acid, contains as much abſolute heat as atmoſpherical air; for the power of the former, in maintaining flame, is nearly as great as that of the latter. Hence we may account for the ſpontaneous accenſion of a mixture of oil of turpentine and nitrous acid.

When the nitrous acid is mixed with oil of turpentine, the inflammable principle is ſeparated from the oil and combined with the acid; the latter is forced to give out a portion of its abſolute heat, part of which is abſorbed by the baſis of the oil, and the reſt becomes redundant or appears in the form of ſenſible heat. If the ſenſible heat be increaſed to a certain degree, the acid will be converted into vapour, and the latter will ſuddenly combine with the inflammable principle of the oil, in conſequence of which a great quantity of fire will be extricated, and the whole will explode with a vivid flame, and with intenſe heat. This exploſion happens in a vacuum as well as in the open air, becauſe the nitrous acid vapour is capable of ſupporting flame.

In the deflagration of nitre, the acid is converted into vapour; which being at the ſame moment united to the inflammable principle of the coal, the fire is inſtantly diſengaged, an elaſtic fluid is ſuddenly expanded, and a loud exploſion produced.

Upon the ſame principle we may account for the heat which ariſes from the mixture of pure and nitrous air; the capacities of theſe fluids being diminiſhed by their combination.

I endeavoured to determine the quantity of heat produced by mixing nitrous and dephlogiſticated air together, in the following manner. Three pipes were inſerted in the braſs veſſel G R. See plate III. fig. 3. The firſt of theſe pipes communicated with an air pump. To the extremity of the ſecond was ſcrewed a bladder containing dephlogiſticated air; and to that of the third, a bladder containing nitrous air, the communication between the air in the bladders and that in the braſs veſſel being interrupted by ſtop cocks. The braſs veſſel was then fixed in the tinned veſſel A D (ſee fig. 4,) and was ſurrounded with water; after which the common air was exhauſted by the air pump, and the nitrous and dephlogiſticated air were ſuffered to enter. From ſeveral trials it appeared that the heat produced by mixing two parts of nitrous with one of dephlogiſticated air, was nearly half as great as that which was produced by burning dephlogiſticated and inflammable air together in the ſame proportions. At the concluſion of the experiments with nitrous and dephlogiſticated air, it was found that an elaſtic fluid remained in the veſſel, which iſſued in the form of ſmoke, and which ſmelled ſtrongly of the nitrous acid. It is probable that a conſiderable quantity of heat was contained in this vapour, which explains the reaſon why leſs heat is produced by the union of theſe fluids, than by that of dephlogiſticated and inflammable air.

This doctrine likewiſe affords an explanation of the heat which is produced by the electric ſpark. For it has been proved by Dr. Prieſtley that the electric ſpark diminiſhes common air, and renders it incapable of ſupporting flame; and the air, when it undergoes this change, is found univerſally to give out a portion of its abſolute heat. It is not probable, however, that the heat which is excited by the electric ſpark entirely originates from this ſource, for the ſpark gives a conſiderable impulſe to the body into which it enters, and we know that percuſſion will produce heat in a vacuum. If a great quantity of electric matter be ſuddenly diſengaged from a cloud or from the earth, it will extricate a proportionable quantity of fire in its paſſage through the air; and thus we may account for the ſudden riſing of the thermometer in the time of thunder and lightning.

It is found, by experiment, that the phenomena of an earthquake may be imitated by a mixture of iron filings and brimſtone, made into a paſte with water, and buried in the earth. May not the heat which is thus produced be explained in the following manner?

When iron filings and brimſtone are mixed together and moiſtened with water, the changes produced in theſe ſubſtances are ſimilar to thoſe which take place when they are burned in the open air. The ſulphur is converted into an acid, and the iron into a calx. The calx and acid are at the ſame time combined with a quantity of air or water, by which their weights are conſiderably augmented. The ſubſtance which is thus united to the calx and acid, muſt either be derived from the common air which is diffuſed through the pores of the earth, or from the water with which the iron and ſulphur were mixed. The latter appears the moſt probable, for the quantity of common air with which the iron and ſulphur are in contact is ſo ſmall that it does not ſeem ſufficient to ſupply them with the fluid which they attract during their union.

When, therefore, ſulphur and iron are mixed together, and made into a paſte with water, it is probable that a part of the water is combined with theſe ſubſtances *. This combination muſt neceſſarily be accompanied with the extrication of a great quantity of ſenſible heat; for the abſolute heat contained in the fluid, which is united to the calx and to the acid, is much leſs than that contained in water in its uncombined ſtate, as appears from a compariſon of the quantity of heat in water, with that which exiſts in the vitriolic acid, and in the calces of metals. The portion of the water, therefore, which is united to the acid and to the calx, muſt have its capacity for heat greatly diminiſhed.

Hence a large quantity of elementary fire will be extricated, and at the ſame moment the remaining part of the water will be converted into vapour, by the expanſion of which a commotion will be excited, accompanied with noiſe and the eruption of flame, reſembling the phenomena of an earthquake.

May not a ſimilar mixture of ſulphureous and metallic bodies be produced in conſequence of the changes which take place in the bowels of the earth? May not theſe bodies be brought into contact with the

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* See Mr. Kirwan's Eſſay on Phlogiſton, p. 102.

water which is diffuſed through the earth's ſubſtance, or lodged in cavities beneath its ſurface? May they not ſuffer the ſame changes that they are found to undergo in the mixture of iron filings and brimſtone? In which caſe a quantity of fire will be diſengaged, and an elaſtic vapour produced, the latter of which, by its ſudden expanſion, will excite a commotion in the bowels of the earth, and will at length force its way through the ſuperincumbent ſtrata.

If much inflammable matter be lodged in the regions where the ſubterraneous fires have been kindled, and if this matter be mixed with atmoſpherical air, or with ſubſtances, which, by the application of heat, produce a fluid that is capable of maintaining fire, the inflammation may be augmented to a prodigious degree, and the rarified vapours may carry along with them in their aſcent, a great quantity of combuſtible materials in an ignited ſtate, by the expoſure of which, the inflammable principle will be diſcharged, and the flame extended through a large tract of air.

In this manner we may probably account for volcanos, thoſe awful inſtances of combuſtion which are exhibited by nature in the foſſil kingdom *.

It appears, upon the whole, that a variety of important effects are produced in the univerſe by the law of the evolution and abſorption of heat.

Vegetables are elaborated by the aſſiſtance of heat and moiſture, from the elements of earth, air, and water, and by the action of the ſolar light, the principles of which the vegetable tribe is compoſed, are intimately combined with inflammable air, and are obliged to reſign a portion of their abſolute heat. In combuſtion, the inflammable air is disjoined from its vegetable baſis, and is combined with the purer part of the common air; and thus thoſe artificial fires are maintained which are ſo neceſſary in the economy of human affairs. In like manner, by the powers of animal life, the inflammable principle is ſeparated from the blood and diſcharged by reſpiration, in conſequence of which a quantity of fire is abſorbed from the air, and heat is communicated to the animal kingdom.

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* The ſame doctrine ſeems to afford an eaſy ſolution of the heat which is produced by fermentation and putrefaction.

The air which was tainted by combuſtion and reſpiration, is again purified by the action of light upon the ſtems and leaves of vegetables *; and if this effect be produced by the ſeparation of the inflammable principle, it follows that vegetation will reſtore to the air that heat which had been detached from it in the proceſſes of reſpiration and combuſtion; and thus the element of fire, by the medium of pure air, will be continually circulating through the animal and vegetable kingdoms.

It was before ſhown that the quantity of air which an animal phlogiſticates in a warm medium is not ſo great as that which it phlogiſticates when it is expoſed to a cold medium. Whence it follows, that in the countries between the tropics, all other circumſtances being equal, the air will be leſs contaminated by the reſpiration of animals, than in the temperate and frigid zones. And the difference between the injury done to the air by reſpiration in warm and in cold climates, will appear to be very conſiderable if we recollect, that the phlogiſtication of a given quantity of air, by an animal

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* See Dr. Prieſtley's Experiments on Air.

at the temperature of 50, was found to be twice as great as that produced by the ſame animal, in an equal time, at the temperature of 100. In thoſe parts of the earth, therefore, in which the contamination of the air is increaſed by the influence of heat upon *dead* animal ſubſtances, that element ſuſtains leſs injury from the reſpiration of *living* animals.

We may add, that in the tropical countries the waſte of pure air by putrefaction, is ſpeedily repaired by a more luxuriant vegetation, and a greater expoſure to light.

Theſe appear to be the principal agents which the great Author of Nature has employed to maintain an uniform degree of purity in the atmoſphere. It is poſſible, indeed, that many other cauſes may concur in producing the ſame effect; but it is evident, from the obſervations which have been made on the ſtate of the air in different parts of the world, that the cauſes which purify and contaminate that element, as well as thoſe which preſerve the balance of heat in the animal kingdom, are adjuſted to each other with admirable preciſion.

I ſhall obſerve, in the next place, that by the law of the evolution and abſorption of heat, the Deity has guarded againſt ſudden viciſſitudes of heat and cold upon the ſurface of the earth.

For if heat were not evolved by the proceſs of congelation, all the waters which were expoſed to the influence of the external air, when its temperature was reduced below 32, would ſpeedily become ſolid; and at the moment of congelation, the progreſs of cooling would be as rapid as it was before the air had arrived at the freezing point.

This is manifeſt from what was formerly obſerved reſpecting the times of the congelation of different fluids: it was ſhown, that if the velocities of the ſeparation of heat were equal, the times of the congelation would be in proportion to the quantities of heat which the fluids gave off from an internal ſource in the freezing proceſs. Whence it follows, that if no heat were evolved, the congelation would be inſtantaneous.

In the preſent ſtate of things, as ſoon as the atmoſphere is cooled below 32, the waters begin to freeze, and at the ſame time to evolve heat; in conſequence of which, whatever may be the degree of cold in the external air, the freezing maſs remains at 32, until the whole is congealed *. And as the quantity of heat extricated in the freezing of water is conſiderable, the progreſs of congelation, in large maſſes, is very ſlow.

Hence, in the northern and ſouthern regions, upon the approach of winter, a quantity of elementary fire is extricated from the waters, proportional to the degree of cold that prevails in the atmoſphere. Thus the ſeverity of the froſt is mitigated and its progreſs retarded; and it would ſeem that during this retardation of the cooling proceſs, the various tribes of animals and vegetables which inhabit the circumpolar regions, gradually acquire the power of reſiſting its influence.

On the contrary, if, in the melting of ice, a quantity of heat were not abſorbed and rendered inſenſible, that ſubſtance, when it

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* That the abſorption and extrication of heat in the melting and freezing of bodies, has a tendency to retard the progreſs of theſe proceſſes is remarked by Mr. Wilkie in his Eſſay on Latent Heat.

The ſame doctrine is likewiſe taught by Dr. Black in his lectures.

was expoſed to a medium warmer than 32, would ſpeedily become fluid, and the progreſs of heating would be as rapid as if no alteration in its form had taken place. If things were thus conſtituted, the vaſt maſſes of ice and ſnow, which are collected in the frigid zones, would, upon the approach of ſummer, ſuddenly diſſolve, and great inundations would annually overflow the regions near to the poles.

But, by the operation of the law of the abſorption of heat, when the ice and ſnow, upon the return of ſpring, have arrived at 32, they begin to melt, and at the ſame time to imbibe heat. During this proceſs a large quantity of elementary fire becomes inſenſible, in conſequence of which the earth is ſlowly heated, and thoſe gradual changes are produced which are eſſential to the preſervation of the animal and vegetable kingdoms.

We may remark, in the laſt place, that this law not only reſiſts ſudden changes of temperature, but that it likewiſe contributes to a more equal diſtribution of the principle of heat, throughout the various parts of the earth, in different ſeaſons and climates. Thus the diurnal heats are moderated by the evaporation of the waters on the earth's ſurface, a portion of the fire derived from the ſun being abſorbed and extinguiſhed by the vapours at the moment of their aſcent. On the approach of night the vapours are again condenſed, and falling in the form of dew, communicate to the air and to the earth, the fire which they had imbibed during the day.

It was before ſhown, that, in the regions near to the poles, when the vernal and ſummer heats prevail, proviſion is made for tempering the ſeverity of the winter colds; a quantity of elementary fire, upon the diſſolution of the ice and ſnow, being abſorbed by the waters, and depoſited as it were in a great magazine, for the purpoſe of mitigating the intenſity of the cold when the froſt returns.

From the Experiments of Hales, Halley, and Watſon, it appears, that vaſt quantities of water are continually converted into vapour, by the action of the ſolar rays upon that portion of the earth's ſurface which is expoſed to the light. And by the celebrated diſcovery of Dr. Black, it is proved, that, in the proceſs of evaporation, much elementary fire is abſorbed. It is manifeſt, that this cauſe will have a powerful influence in mitigating the intenſity of the heat, in the torrid zone, and in promoting a more equal diffuſion of it throughout the earth. For a conſiderable portion of the heat, which is excited by the action of the ſolar rays upon the earth's ſurface, within the tropics, is abſorbed by the aqueous vapours, which being collected in the form of clouds, are ſpread like a canopy over the horizon, to defend the ſubjacent regions from the direct rays of the ſun. A great quantity of elementary fire is thus rendered inſenſible in the torrid zone, and is carried by the diſperſion of the vapours to the north and to the ſouth, where it is gradually communicated to the earth when the vapours are condenſed.

It was before obſerved, that the doctrine which is advanced in the preceding pages, with reſpect to the cauſe of animal heat and of combuſtion, is the reſult of the general fact, that the changes which are produced in the temperatures of different bodies, by the application of given quantities of heat, are different; or, that, the quantities of matter being equal, the ſame quantity of heat which raiſes *one* body a certain number of degrees, will raiſe *another*, a greater or a leſs number, according to the nature of the body to which it is applied. This fact ſeems to have been ſufficiently verified by experiment; and, therefore, the conſequences which have been deduced from it, do not appear to be connected with any hypotheſis reſpecting the nature of heat.

For this reaſon, I have not entered into the enquiry, which has been ſo much agitated among the Engliſh, the French, and the German philoſophers, whether heat be a *ſubſtance* or a *quality*. It is true, I have, in ſome places, made uſe of expreſſions, which ſeem to favour the former of theſe opinions. But my ſole motive for adopting this language, was, becauſe it appeared to be more ſimple and natural, and more conſonant to the facts which had been eſtabliſhed by experiment. At the ſame time, I am perſuaded, that it will be found to be a very difficult matter to reconcile many of the phenomena with the ſuppoſition, that heat is a quality. It is not eaſy to conceive, upon this hypotheſis, how heat can be abſorbed in the proceſſes of fuſion, evaporation, combuſtion; how the quantity of heat in the air can be diminiſhed, and that in the blood, increaſed, by reſpiration, though no ſenſible heat or cold be produced.

Whereas if we adopt the opinion, that heat is a diſtinct ſubſtance, or an element *ſui generis*, the phenomena will be found to admit of a ſimple and obvious interpretation.

Fire will be conſidered as a principle, which is diſtributed in various proportions throughout the different kingdoms of nature. The mode of its union with bodies, will reſemble that particular ſpecies of chemical union, wherein the elements are combined by the joint forces of preſſure and of attraction. Of this kind is the combination of fixed air and water; for fixed air is retained in water partly by its attraction to that fluid, and partly by the preſſure of the external air; and, if either of theſe forces be diminiſhed, a portion of the fixed air eſcapes. In like manner, it may be conceived, that elementary fire is retained in bodies, partly by its attraction to thoſe bodies, and partly by the action of the ſurrounding heat; and, in that caſe, a portion of it will be diſengaged either by diminiſhing the attractive force, or by leſſening the temperature of the circumambient medium. If this be admitted, it will follow, that, by the alterations which the air and blood undergo in the proceſs of reſpiration, the attraction of the former to fire is diminiſhed, and that of the latter increaſed, in conſequence of which the fire paſſes from the air to the blood; and that, in combuſtion, the fire is ſeparated from the air by the action of the inflammable principle, in the ſame manner as the nitrous acid is detached by the vitriolic, from an earth or alkali.

If, however, fire be a ſubſtance which is ſubject to the laws of attraction, the mode of its union with bodies ſeems to be different from that which takes place in chemical combination. For, in chemical combination, the elements acquire *new* properties, and either wholly, or in part, loſe thoſe by which they were formerly characterized. But I have before endeavoured to ſhow, that we have no ſufficient evidence for believing that fire, in conſequence of its union with bodies, does, in any inſtance, loſe its diſtinguiſhing properties.

I may add, in the laſt place, that a table ſhould be formed, exhibiting the comparative quantities of fire contained in all known bodies.

Inflammable air ſhould be placed at the head of this table, under which ſhould ſtand, dephlogiſticated and atmoſpherical air—the vapour of the nitrous acid, and probably of ſome other fluids—arterial blood, water, &c.

Much time, and a ſeries of accurate experiments, are ſtill required, in order to the complete inveſtigation of this ſubject. There are few ſubjects, however, which have a better claim to the attention of Philoſophers.

The proper adjuſtment of heat is of the utmoſt moment to the health and life of animals. An enquiry, therefore, into the cauſes which maintain the balance of heat in the animal œconomy, will probably give riſe to improvements in the prevention and cure of diſeaſes.

Many of the chemical arts wholly depend upon a nice regulation of heat. It is reaſonable to believe, that thoſe arts may be improved, and the conveniencies of life multiplied, by an attention to the theory and operations of fire.

The proſecution and advancement of this ſubject is, moreover, calculated to extend our knowledge of the ſyſtem of the world; for the operations of the element of fire hold a principal place in the ſeries of cauſes, by which the adjuſtment of the univerſe is maintained. As the law of gravity, combined with the projectile force, regulates the motions of the great bodies which compoſe the planetary ſyſtem; ſo it is the repulſive force of fire, combined with corpuſcular attraction, that keeps the ſmaller particles of matter upon the ſurface of the earth, and probably throughout the univerſe, in a ſtate of conſtant fluctuation; and there is little doubt that the improvements of chemiſtry will, at ſome future period, diſcover the ſame exquiſite contrivance in the arrangement of the more minute parts of Nature, which the improvements of natural philoſophy have ſhown to prevail in the planetary ſyſtem.

I ſhall farther obſerve, that ſuch ſpeculations have a direct tendency to influence the moral character of man. It is this, indeed, which ſtamps them with their principal value; for all the other improvements, which may be ſuppoſed to ariſe from the cultivation of Nature, if they were unaccompanied with a correſponding advancement in morals, could ſcarcely be conſidered as bleſſings. It is ſurely of very little moment to us, that we ſhould procure the means of gratifying our deſires, if we do not at the ſame time acquire the habit of regulating them. Could the increaſe of power be deemed a benefit, if it were uſed as a ſcourge; or of wealth, if it were made the inſtrument of corruption; or of knowledge, if it were employed to deceive?

The inveſtigation of Nature is, however, peculiarly calculated to inſtruct us in that moſt important of all arts, the art of living. To whatever height we may aſcend in tracing the cauſes, which regulate the ſyſtem of the world, our views muſt at laſt terminate in an uncauſed Being, in whom all the beauty and order, all the wiſdom and power diſplayed throughout the univerſe, are centered. Every ſtep which we advance in the cultivation of natural knowledge, contributes more firmly to eſtabliſh this great truth; becauſe it affords an additional proof of exquiſite ſkill and contrivance in the formation of the world. By thus ſhowing us the Deity in his works, the contemplation of nature leads to conſequences, in the higheſt degree intereſting to intellectual and moral agents. When we look around us, we perceive that every part of the material world is governed by general laws; and when we reflect that, in this vaſt ſyſtem of things, a race of Beings exiſts, to whom the Deity has communicated a portion of his intelligence and activity, we cannot avoid concluding, that laws muſt have been ordained for the government of ſuch Beings, as well as for that of all other parts of the univerſe.

Indeed, an attentive obſervation of human Nature, clearly points out the hand of the Deity, preſcribing limits to the intellectual, as well as to the material world; for, in the original feelings and judgments of the human mind, there are evident traces of a moral law, which is antecedent to all poſitive inſtitutions, and which muſt conſequently have been written on the heart of man by the ſpirit of the Almighty.

Moreover, in contemplating the plan of the univerſe, we perceive that the whole is arranged into one great ſyſtem; that an uninterrupted chain of connection extends throughout the various claſſes of Beings;

that nothing in Nature is quieſcent; that the paſt has been ſubſervient to the preſent, as the preſent will be to the future; in a word, that all things are in a ſtate of progreſſion, and apparently tend towards the accompliſhment of a great deſign, which ſeems to have influenced the whole ſeries of events ſince the creation, and which probably will not be completed, till the preſent ſcene of things is cloſed by the diſſolution of the material world.

If we compare the various claſſes of Beings, which fall within the reach of our obſervation, we perceive that intelligent and moral agents hold the higheſt rank; and, conſequently, of all the laws which regulate the ſyſtem of Nature, thoſe that have been eſtabliſhed for the purpoſe of maintaining harmony and order in the intellectual world, muſt be the moſt important.

It is thus that a juſt eſtimate of the value of the ſeveral objects which preſent themſelves to our view, in a general ſurvey of the univerſe, informs us, that obedience to the moral law conſtitutes the ſupreme perfection of the moſt exalted orders of created Beings; and as conſummate moral excellence is the higheſt point of eminence, which the imagination can conceive an intellectual nature to attain, we are led to conſider this as the firſt attribute of the Deity.

Hence, in inveſtigating the final cauſes of the ſyſtem of the world, there are none which appear more important, or more worthy of an infinitely perfect mind, than that the univerſe ſhould be conſidered as formed for the purpoſes of communicating happineſs to ſenſitive Beings, and of training intellectual and moral agents in the rudiments of wiſdom and virtue.

Indeed, the beſt deductions of human reaſon concur with revelation in aſſuring us, that the attainment of moral excellence is the principal object of the creation. This it is which gives unity and perfection to the whole. We ſee that a great quantity of preſent happineſs is continually ſacrificed to it; that it is frequently purſued through danger, and difficulty, and diſtreſs; nay, that on ſome occaſions it is ſought after in the midſt of commotions, which convulſe the frame of Nature, and which carry in their train deſolation and ruin. In fine, we have reaſon to believe, that the advancement of moral excellence is the ultimate point, towards which the movements of this vaſt ſyſtem are directed; and, as it ſeems to have been the great final cauſe which gave riſe to the creation of the material world, ſo it is probable, that it will be the principal aim of the Deity in its diſſolution.

The tendency of ſuch ſpeculations to influence the human character is obvious. In an enlightened age, the ſtate of morals among mankind, principally depends upon the direction which is given to the collective exertions of genius, and of intellectual ability. If the efforts of genius and of intellect be concentrated upon objects which are uſeful and important, and particularly upon thoſe which enlarge and elevate the mind, the manners of the people will be virtuous; and if the reverſe, they will be depraved. In the cultivation of natural knowledge, the intellectual faculties of man are employed in inveſtigating the laws which regulate the courſe of Nature, and in tracing them to their divine original; and nothing ſurely can have a greater tendency to elevate and enlarge the mind, and to add weight and dignity to the moral law, than reſearches which terminate in that auguſt Being, who is the primary cauſe of exiſtence, and of motion in the material world, and to whom the various orders of intellectual and moral agents, throughout the univerſe, are related as their Creator, their Lawgiver and Judge.

APPENDIX.

AS it may perhaps be imagined that the differences between the heats imparted by pure and common air, in the experiments recited in the foregoing work, were ſo ſmall that they could not be diſtinguiſhed with ſufficient certainty; I think it proper to obſerve, that I have repeated thoſe experiments with thermometers that have each degree of Farhenheit's ſcale, divided into 50 equal parts. With a view to prevent the eſcape of the heat, each of the tinned veſſels G H, I K, (ſee plate II.) was ſurrounded with another tinned veſſel, which exceeded it in breadth nearly one inch, and the intermediate ſpace was ſtuffed with fine down. The mouths of the interior veſſels were cloſely covered by lids, which were lined with thick flannel.

The thermometers were inſerted in the veſſels to ſuch a depth that their bulbs ſhould reach a little lower than the center.

An apparatus was contrived for agitating the water when the veſſels were covered, and the lids were formed in ſuch a manner that they could be applied, after the cylinders were immerſed.

For the purpoſe of trying the accuracy of the apparatus, equal quantities of common air were introduced into the cylinders deſcribed in page 178. The cylinders were heated in the manner formerly explained, (ſee p. 186.) They were immerſed in the tinned veſſels, containing equal quantities of water, at the temperature of the atmoſphere, and the mouths of the veſſels were cloſed to prevent the acceſs of the external air. Being ſuffered to remain in this ſituation during 12 minutes, they were removed; the veſſels were again covered; the water was agitated; and the heats communicated were accurately obſerved.

This experiment was frequently repeated with common air in both cylinders, and it appeared that the deviation, in the reſults of the different trials, was two degrees, as eſtimated by the above-mentioned thermometers, or $\frac{2}{50}$ of a degree of Farhenheit.

Dephlogiſticated air was then introduced into one of the cylinders, and common air into the other; and it was found that when the former was nearly of the ſame ſtandard with that uſed in Experiment II. p. 190, the exceſs of the heat which it imparted, above that communicated by the common air, was eight degrees; and when the pureſt dephlogiſticated air was uſed, the exceſs was nearly 13 degrees *.

It is proper to obſerve, that the agitation of the water was eſſential to the ſucceſs of this experiment; for though the bores of the thermometers were very fine, yet, in order to obtain a large ſcale, it was neceſſary that the bulbs ſhould be of a conſiderable ſize.

And hence when one of theſe inſtruments was immerſed in water, which had a higher temperature than itſelf by three or four de-

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* The reſults of theſe experiments nearly correſpond with thoſe which have been recited above. (See p. 190.) For the difference in favour of dephlogiſticated air, by my former trials, was $\frac{2}{10}$ of a degree of Farhenheit; and eight degrees, as meaſured by the thermometers, which were uſed in my laſt experiments, are equal to $\frac{2}{10}$ of a degree of Farhenheit minus $\frac{1}{25}$.

grees; if the water was kept in a ſtate of reſt, an interval of ſeveral minutes elapſed before the thermometer arrived at the exact temperature of the fluid with which it was in contact; but if the water was moderately agitated, the thermometer acquired the ſame temperature with it in leſs than half a minute.

The ſcales of theſe thermometers were divided upon thin braſs plates, which were fixed to flat pieces of wood. The degrees were rendered much more diſtinct by painting black the portion of the tube that was in contact with the ſcale; an improvement which was firſt ſuggeſted to me by my worthy friend Mr. Benjamin Vaughan.

The propoſition recited in page 88; that the comparative quantities of *abſolute* heat in bodies, are reciprocally proportional to the changes produced in their *ſenſible* heats, when they are mixed together at different temperatures, may be proved geometrically as follows. See figures 3 and 4, plate IV. Let *a b c d*, *e f g h* repreſent the quantities of heat contained in equal weights of two heterogeneous bodies A and B, which have a common temperature. The temperatures will be expreſſed by the equal lines dc, ef, and the capacities by ad, eb. Let B be raiſed to a higher temperature than A, and let its increaſe of temperature be expreſſed by the line ek, the augmentation of its abſolute heat will be expreſſed by the figure kl. Let the bodies now be mixed together, and when they have been brought to a common heat, let the diminution of temperature in B be expreſſed by the line ki, and the increaſe of temperature in A, by the line do. do and ki will be reciprocally proportional to the quantities of heat in A and B, when they are brought to a common temperature. For km expreſſes the quantity of abſolute heat which is ſeparated from B, and nd that which is added to A; therefore $nd = km$, and conſequently $ki : od :: no : im$. But, becauſe the figures nc and ig have the ſame altitude, it will be as $no : im :: nc : ig$; therefore as $nc : ig :: ki : od$. That is, the quantities of abſolute heat in A and B, when they are brought to a common temperature, are reciprocally proportional to the variations in their ſenſible heats, when they are mixed together at different temperatures.

The comparative heats expreſs the quantities of abſolute heat when the weights and temperatures are equal. See definition, p. 3. And ſince A and B have equal temperatures and weights, it follows, that $n\,c$ and $i\,g$ may be uſed to expreſs the comparative heats.

Hence, if we call the comparative heats of A and B, C and c; and if we call the variation of temperature in the former x, and in the latter y, it will be as $C : c :: y : x$; therefore $C = \frac{c\,y}{x}$. By this equation the comparative heats of bodies may be calculated when the quantities of matter, employed in the experiments, are equal.

If the quantities of matter be unequal, the equation will be as follows: The capacities are proportional to the comparative heats. See definition, p. 7.

Let C expreſs the capacity, or the comparative heat.
T the temperature.
A the abſolute heat.
Q the quantity of matter.

It was before ſhown, page 11, that if Q be given, A is as C × T; and if C and T be given, A will be as Q. That is, if the capacities and temperatures be equal, the quantities of abſolute heat will be as the quantities of matter. This appears from the obſervations in page 17. If, therefore, the capacities, the temperatures, and the quantities of matter vary; that is, if neither C, T, nor Q be given, in that caſe A will be as C × T × Q. Conſequently C will be as $\frac{A}{TQ}$, and if A be given, C will be reciprocally as T Q. That is, if the quantities of abſolute heat be equal, the capacities will be reciprocally as the temperatures multiplied into the quantities of matter.

Let C and c be the capacities of two bodies, A and B.
q and Q the quantities of matter.
T and t the temperatures.

It will be $C : c :: Qt : qT$, and therefore $C = \frac{cQt}{qT}$.

The reader will obſerve, that as the capacities are ſuppoſed to be permanent, t and T expreſs univerſally the variations of temperature which are produced by the addition or ſeparation of equal quantities of fire; for theſe variations, multiplied into the quantities of matter, will always be reciprocally proportional to the capacities; and ſince when two bodies are mixed together at different temperatures, the fire which is ſeparated from the warmer is the ſame with that which is added to the colder, t and T may be uſed to expreſs the variations of temperature produced by the ſeparation and addition of this fire. Hence t and T have the ſame value with y and x in the foregoing equation, and therefore $C = \frac{c\,Q\,y}{q\,x}$. By this equation the comparative heats may be calculated when unequal quantities of matter are employed in the experiments.

An equation for calculating the loweſt degree of heat may be obtained as follows: See fig. 5, plate IV. Let $e\,b$ repreſent the whole quantity of fire contained in a body A, and let $e\,g$ repreſent its capacity, and $e\,f$ its temperature eſtimated from the loweſt point. Let this body be ſuppoſed to have its capacity diminiſhed by a change of form, and after the change let the capacity be repreſented by the line da, its temperature by dc, and the quantity of fire which it contains by db. If the body be ſuppoſed not to be in contact with any other body which can communicate elementary fire to it, or ſeparate that principle from it, the quantities of fire before and after the change will be equal; that is, db will be equal to eb, and $dg = ec$, therefore $eg - gb : gb :: db : bc$; that is, the difference of the capacities before and after the change, is to the capacity, after the change, as the ſenſible heat produced, is to the whole number of degrees of heat which the body contained, previouſly to the alteration in its form.

Let therefore C expreſs the capacity before the change.
c the capacity after the change.
l the ſenſible heat produced.
S the whole number of degrees of heat (previouſly to the change) eſtimated from the point of total privation.

Then $C - c : c :: l : S$.

Therefore $S = \dfrac{cl}{C - c}$.

By this equation we may calculate the loweſt degree of heat, if the ſenſible heat, which a body produces, in conſequence of a change of form, and the capacities before and after the change, be given.

There is reaſon to believe, from the experiments of Dr. Irvine, that the ſenſible heat or cold produced by mixing bodies together at the ſame temperature, in conſequence of a chemical action, ariſes from a diminution, or an increaſe, in the ſum of the capacities. The loweſt degree of heat, as deduced from ſuch experiments, may be calculated in the following manner;

1. Let the quantities of matter mixed together be equal.

Let $C + c$ be the ſum of the capacities before the mixture.
$K + k$ the ſum of the capacities after the mixture.

Then $C + c - \overline{K + k} : K + k :: l : S$

and $S = \dfrac{\overline{K + k} \cdot l}{C + c - K - k}$.

2. Let the quantities of matter mixed together be unequal, and let Q and q be the quantities of matter.

Then $CQ + cq - \overline{KQ + kq} : KQ + kq :: L : S$ and $S = \frac{\overline{KQ + kq}\,.\,L}{CQ + cq - KQ - kq}.$

Hence, if two bodies, which produce ſenſible heat or cold by chemical action, be mixed together at the ſame temperature, and if the quantities of matter, the capacities before and after the mixture, and the ſenſible heat or cold produced, be given by experiment, the loweſt degree of heat will be known by calculation.

It was before obſerved, that the queſtion reſpecting the chemical combination of fire with bodies would be reſolved, if a ſeries of accurate experiments were inſtituted, in order to diſcover the changes of capacity which ariſe from alterations of form in different bodies, together with the quantities of heat and cold produced by thoſe alterations.

If it be found that the loweſt degree of heat, as calculated from ſuch experiments is conſtant, it will follow that heat does not, in any caſe, enter into a chemical union with bodies, and conſequently that the ſenſible heat or cold which they produce, when they undergo a change of form, ariſe ſolely from alterations of capacity; but if the loweſt degree of heat be not conſtant, we may infer that a part of the heat, which is abſorbed by bodies, when their capacities are increaſed, enters into a chemical union with them, and loſes its diſtinguiſhing properties.

The following ſeries of well conducted experiments, which were communicated to me by Mr. Gadolin, Profeſſor of Chemiſtry at Abo, in Sweden, ſeem to afford a ſtrong preſumption that the loweſt degree of heat is conſtant.

Reſults of Mr. Gadolin's Experiments on the abſolute heat of Bodies, publiſhed in the Memoirs of Stockholm for 1784.

The capacity of ſnow is ſtated by Mr. Magellan, in his Eſſay on Fire, to be to that of water as 9 to 10. Taking this for granted, I, by direct experiments, found that ſnow, in melting, deprives an equal weight of water of about 80 degrees of heat according to Celſius's thermometer. This being equal to $\frac{1}{10}$ of the whole quantity of heat contained in water at the melting point, I concluded that this whole quantity is equal to 800 degrees according to Celſius, or that the loweſt point of the thermometric ſcale is ſituated at the 800th degree below o. This converted into degrees of Farhenheit, gives 1408 degrees below o for the place of the loweſt point *.

By direct experiments I alſo meaſured the capacity of common ſalt, as well as its ſolutions in different proportions of water. Thus I found the comparative heat of criſtalliſed common ſalt to be - 0,226,
that of a ſaturated ſolution (containing 372 parts of ſalt, united with 1000 parts of water) - 0,793,
that of a ſolution containing ſalt and water in the proportion of 3 to 10 0,820

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* The experiments made by me, at that time, gave 80,6 degrees for the heat abſorbed by ſnow during its converſion into water, accordingly the number of degrees from o to the loweſt point ſhould have been 806 according to Celſius, or 1419 of Fahrenheit; however I preferred 800 as a more even number. In repeating thoſe experiments I have afterwards found that the heat abſorbed is ſomewhat greater, viz. 81,2 degrees according to Celſius; whence, ſuppoſing the proportion of the capacity to be the ſame as 9 : 10, it would follow that the abſolute degrees are 812 below o of Celſius's thermometer, or 1430 below o of Fahrenheit.

that of a ſolution -	2 to 10	0,866
———————	1,56 to 10	0,905
———————	1 to 10	0,936
that of water being ſuppoſed equal to - -		1,000

To find out the comparative heats of the ſaline ſolutions, I made uſe of a piece of copper, which I either heated or cooled in thoſe ſolutions. As for the criſtalline common ſalt, I tried it by mixing it, when finely powdered, with a ſaturated ſolution.

To compare the difference of the comparative heats with the colds produced during the ſolutions, I made the following experiments.

Exp. I. With 10 parts of water, contained in a tinned veſſel whoſe capacity was $= 0,6$, their common temperature being $= 20°$ (Celſius's,) five parts of common ſalt reduced to a fine powder, whoſe temperature was 21,°2, were mixed. After the ſolution the common temperature was found to be 13,°58.

Here the whole quantity of heat, before the ſolution, was equal to $10 \times 820,8 + 0,6 \times 820,8 + 5 \times 0,226 \times 821,2$; and the whole quantity of heat after the ſolution, ſuppoſing the capacity of the ſaturated ſolution = X, will be thus expreſſed *.

$$13{,}72 \times X \times 818{,}58 + 1{,}28 \times 0{,}226 \times 818{,}58 + 0{,}6 \times 818{,}58.$$

If, during the ſolution, no part of the heat was chemically bound, this laſt expreſſion muſt be equal to the former; which being ſuppoſed, the value of X will be found equal to

$$X = \frac{10{,}6 \times 820{,}8 + 5 \times 0{,}226 \times 821{,}2 - 1{,}28 \times 0{,}226 \times 818{,}58 + 0{,}6 \times 818{,}58}{13{,}72 \times 818{,}58}$$

= 0,7926; which exactly coincides with the comparative heat found out for the ſaturated ſolution, 0,793.

The ſame experiment was repeated thus:

Exp. II. In a veſſel, whoſe capacity was 0,7; 14,5 parts of water were mixed with 5,5 parts of ſalt; the common temperature before the mixture being 19°,5. The common temperature after the ſolution was 16°,95. If the calculation from this experiment be inſtituted as from the foregoing, we find X = 0,7927.

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* As 10 parts of water are able to diſſolve 3,72 parts of common ſalt, it follows that the maſs, after the mixture, contains 13,72 parts of ſaturated ſolution, and 1,28 parts of undiſſolved common ſalt.

Exp. III. 3,5 parts of common ſalt was put into a glaſs veſſel, whoſe capacity was 1,2, their common temperature being 23°. To this was poured 10 parts of water, heated to 40°,84; and the common temperature after the mixture was found to be 35°,65.

From this experiment the capacity of a ſolution of 3,5 parts of ſalt, in 10 parts of water, may be computed, and it will be found equal to 0,802; that is, leſs than the capacity of a ſolution, containing three parts of ſalt, and 10 of water, but greater than the capacity of a ſaturated ſolution.

Exp. IV. In the veſſel, whoſe capacity = 1,2; 4,5 parts of common ſalt were mixed with 15 parts of water, the common temperature before mixture being = 23°; that after mixture was 20,54. If the capacity of this ſolution be computed from this experiment, it will alſo be found very nearly equal to 0,824.

Exp. V. Two parts of ſalt and 10 parts of water were mixed together in a veſſel, whoſe capacity = 0,7; the common temperature before mixture was 20,51, and that after ſolution 18,65; whence the capacity of the ſolution will, by computation, be found = 0,873; the obſerved was 0,866.

Exp. VI. Ten parts of water were mixed with one part of ſalt, in a veſſel whoſe capacity was = 1,2. The temperature before mixture was 21,53, and that after mixture 20,25. The capacity of the ſolution calculated from this experiment will alſo be nearly = 0,932.

Exp. VII. To the ſolution (Experiment VI.) remaining in its veſſel one part more of the ſalt was added, the common temperature being 21°,52; the temperature after the mixture was 20°,99. Hence, ſuppoſing the comparative heat of the weak ſolution = 0,932, that of the new ſolution will by computation be found = 0,873 nearly; which agrees with what was computed from Experiment V.

Exp. VIII. To the ſolution made in the Experiment VII. one part of ſalt was added, the temperature before the mixture was 21°,75, and after the ſolution 21°,45. Hence the capacity of the weaker ſolution being ſuppoſed = 0,873, that of the new made, containing three parts of ſalt united with 10 of water, will by computation be found = 0,824, agreeably to the calculation made from Experiment IV.

To try how the mixture of ſalt ſolutions with water would agree with thoſe obſervations, I made the following experiments.

Exp. IX. Ten parts of ſaturated ſalt ſolution was mixed with 19,7 parts of water, the temperatures of both being = 19°,6; the temperature of the mixture was found = 18°,95.

If the capacity of the ſaturated ſolution is 0,793, that of the diluted in theſe two experiments, (which conſiſts of one part of ſalt and 10 parts of water) will by computation be found = 0,932; which coincides with the reſult of the calculation made from Experiment VI.

Exp. X. Equal parts of ſaturated ſolution and water were mixed together at the temperature of 21°,4; the temperature of the mixture was 20°,75. This mixture contained 1,56 parts of ſalt diſſolved in 10 parts of water, and its capacity calculated from the experiment, ſuppoſing the capacity of the ſaturated ſolution = 0,793, will be found = 0,896. The capacity of an equally diluted ſolution was by direct experiments found to be = 0,905.

The following table exhibits the reſults of the foregoing experiments in one view.

The experiments recited.	Proportion of ſalt and water.	Computed capacity.	Obſerved capacity.	Difference.
1, 2,	3,72 : 10	0,793	0,793	0,000
3,	3,5 : 10	0,802	——	——
1, 4, 8,	3 : 10	0,824	0,820	+ 0,004
5, 7,	2 : 10	0,873	0,866	+ 0,007
10	1,56 : 10	0,896	0,905	— 0,009
6, 9,	1 : 10	0,932	0,936	— 0,004

In all theſe experiments the comparative heats of the mixtures, calculated from the comparative heats of the mixed bodies, and their changed temperatures, agree perfectly with one another, and very nearly with the obſerved comparative heats. It, therefore, ſeems to be beyond all doubt that the colds produced are intirely owing to the changes of the comparative heats, and that no part of the heat here has been chemically combined.

With a view more fully to inveſtigate this ſubject, I thought it proper to make ſimilar experiments with oil of vitriol and water, as the heat produced during their mixture is very conſiderable.

By direct experiments I found the capacities of concentrated dephlogiſticated oil of vitriol to be - - 0,339,

Of four parts of concentrated oil of vitriol mixed with one part of water - - 0,442,

Of concentrated oil of vitriol and water mixed in the proportion of 2 to 1, - - 0,500,

Of concentrated oil of vitriol and water mixed in proportion of

- - 1 to 1, 0,605,
———————— 1 to 2, 0,749,
———————— 1 to 5, 0,876,
———————— 1 to 10, 0,925.

In theſe experiments the acid was included in a thin glaſs bowl, which, being heated in warm water to a certain degree, was cooled in a given quantity of cold water. The capacity of the glaſs bowl, as well as that of the veſſel, was by previous experiments found out, and due corrections accordingly made.

The heats produced during the mixtures were obſerved in the following experiments.

Exp. I. Four parts of concentrated oil of vitriol were mixed with one part of water, their temperature before the mixture being 12°, that after the mixture was 119°,5.

Here the heat before the mixture (the capacity of the concentrated acid being ſuppoſed = 0,339) is $= \overline{4 \times 0,339 + 1} \times 812$; and that after the mixture (the capacity of the mixture being called *X*) is $= 5 \times X \times 919,5$; therefore, if no alteration in the quantity of the heat took place during the mixture, theſe expreſſions muſt be equal; from thence X = 0,416, the obſerved comparative heat of this diluted acid was 0,442; and if in the equation 0,442 be placed inſtead of X, the value of the capacity of the ſtrong acid will, by calculation, be found equal to 0,372, inſtead of 0,339.

Exp. II. Two parts of concentrated acid were mixed with one part of water at the temperature of 11°,13. The quickſilver in the thermometer roſe to the 124th degree. Hence, if the capacity of the concentrated acid be ſuppoſed = 0,339, that of the diluted will, by computation, be found = 0,491; the obſerved was 0,500. Again, if the capacity of the diluted acid in this experiment be ſuppoſed = 0,500, that of the concentrated will, by calculation, be found = 0,354.

Exp. III. Equal parts of concentrated acid and water were mixed together at the temperature of 10°,5; the mixture acquired a temperature of 100°. Hence, ſuppoſing the capacity of the concentrated acid = 0,339, that of the diluted will, by computation, be found = 0,603. Its obſerved capacity was 0,605; and if this be ſuppoſed to be the true capacity, the computed capacity of the concentrated acid is 0,344: here, therefore, the deviation is very inconſiderable.

Exp. IV. One part of concentrated acid being mixed with two parts of water at the temperature of 10°,25, the temperature of the mixture was 70°. By aſſuming the capacity of the concentrated acid = 0,339, that of the diluted will, by computation, be = 0,726; but by aſſuming this = 0,749 (as it was obſerved) that of the concentrated will, by computation, be = 0,413.

Exp. V. One part of concentrated acid being mixed with five parts of water, at the temperature of 10°,25, that of the mixture was 38°,25. If the capacity of the concentrated acid is ſuppoſed = 0,339, that computed for the diluted is 0,860; and the obſerved was 0,876, which being ſubſtituted, that of the concentrated will be computed to 0,438.

Exp. VI. One part of concentrated acid being mixed with 10 parts of water, at the temperature of 9°,5, the temperature of the mixture was 24°,75. Hence the computed capacity of the mixture is 0,923, if that of the concentrated acid is ſuppoſed = 0,339; but if the former is ſuppoſed = 0,925, the computed capacity of the concentrated acid will become = 0,367.

As in theſe experiments the computed capacity of the mixtures conſtantly are leſs than the obſerved ones, and on the other ſide the computed capacity of the ſtrong acid conſtantly greater than the obſerved, one may be apt to think that here really by the mixtures new quantities of heat are produced, that were not to be found in the mixed liquids, before the mixtures, if not chemically combined therein. But when I conſider that this diſcordance is likewiſe a conſequence of the error that my manner of obſerving the comparative heats of the acids might have led into, I think it is more likely ly to attribute the anomaly to this cauſe. Then as the conducting power of concentrated vitriolic acid is very weak in proportion to that of water or a weaker acid, it may eaſily be ſuppoſed that the capacity obſerved for the concentrated acid was greater than what was found by direct obſervation. That the whole deviation of the calculated from the obſerved capacities is owing to ſome errors in the latter, is ſo much the more probable, as in ſome of the following experiments, (Experiments VII. VIII. IX.) the deviations have fallen out in a different way quite contrary to the opinion of a new produced heat.

Exp. VII. Five parts of diluted vitriolic acid (conſiſting of four parts of concentrated acid, and one part of water) were mixed with three parts of water, at the temperature of 10°,5; the temperature of the mixture was 49°. If now the capacity of the acid employed, be ſuppoſed = 0,442, that of the mixture will, by computation, be found = 0,622; but the obſerved capacity of this acid, conſiſting of equal parts of concentrated acid and water was 0,605; and if this be ſuppoſed to be its true value, the capacity of the acid uſed, will, by computation, be = 0,414, *viz.* leſs than what was obſerved.

Exp. VIII. 0,128 parts of the ſame acid (uſed in Experiment VII.) were mixed with one part of water at the temperature of 11°,15; the temperature of the mixture was 18,77. The computation gives 0,928 for the capacity of the mixture, if that of the uſed acid is ſuppoſed = 0,442; but if the former is ſuppoſed = 0,925, the latter will, by computation, be found equal to 0,416.

Exp. IX. 0,158 parts of acid (conſiſting of two parts of concentrated acid, and one part of water) were mixed with one part of water at the temperature of 10°5; that of the mixture was 15°,5. Here the capacity of the mixture will be found = 0,928, and that of the uſed acid 0,492, when calculations are made, ſuppoſing in the former caſe that of the latter acid to be 0,500, and in the latter, the capacity of the mixture to be 0,9 5.

Exp. X. Two parts of acid (conſiſting of equal parts of concentrated acid and water) being mixed with one part of water, at the temperature of 12°; that of the mixture was 19°,43. Hence the computed heats of the uſed acid and the mixture are 0,634, 0,730; the obſerved capacities were 0,605, 0,749.

Exp. XI. One part of the ſame acid (as that uſed in Experiment X.) being mixed with two parts of water, at the temperature of 12°,6, that of the mixture was 17,6: here the computed capacities are 0,644, 0,863; the obſerved were 0,605, 0,876.

Exp. XII. Two parts of the ſame acid (Experiment X.) being mixed with nine parts of water, at the temperature of 12,4; that of the mixture was 15°,15. Therefore the computed capacities are 0,605, 0,925; which perfectly coincides with the obſerved ones.

Exp. XIII. One part of diluted vitriolic acid (conſiſting of one part of concentrated acid, and two parts of water) being mixed with one part of water, at the temperature of 12°,25; the temperature of the mixture was 14°. Hence, if the capacity of the uſed acid is ſuppoſed = 0,749; that of the mixture will, by computation, be found = 0,873; and, again, if the latter is ſuppoſed = 0,876, the former will be = 0,756.

The differences between the calculated and obſerved capacities are not greater than thoſe which may be ſuppoſed to have ariſen from the errors committed in finding out the latter; nor can they eaſily be accounted for otherwiſe, as they are ſomewhat irregular, the exceſſes being ſometimes on one, ſometimes on the other ſide. However it may be a little more ſatisfactory to point out the capacities corrected by approximation from the experiments mentioned.

The capacities may be more nearly approximated, by taking a medium between the value of the capacity of each acid, found by computation from the experiments made with it, and its value pointed out by direct obſervation. Thus I find the corrected capacities to be the following.

The capacity of concentrated acid		0,360
Of concentrated acid and water in the proportion of -	4 : 1	0,429
———————	2 : 1	0,496
———————	1 : 1	0,613
———————	1 : 2	0,743
———————	1 : 5	0,871
———————	1 : 10	0,925

Theſe values agree, I think, very nearly with the true capacities of the acids. I will, therefore, ſuppoſe them to be the true values, and, from this ſuppoſition, make calculations for the heat that ought to have been produced by the mixtures made in the abovementioned experiments, if this heat was cauſed only by the changes of capacities; that is, if the whole quantity of heat was the ſame before and after the mixture; the loweſt point of the thermometer at 800° below 0; and if the capacities of the concentrated and diluted acids exactly agree with the values juſt exhibited.

Then I find that the temperature of the mixture, in the 1ſt Experiment, ought to have been, 123°,7 inſtead of 119°,5 as obſerved.

2d. Exp.	137,6	-	124
3d. Exp.	99,1	-	100
4th. Exp.	57,9	-	70
5th. Exp.	31	-	38,25
6th. Exp.	24,22	-	24,75
7th. Exp.	50,33	-	49
8th. Exp.	20,1	-	18,77.

In the other experiments, the heats produced are ſo ſmall, that they cannot be expected to be determined by calculation, with any degree of preciſion; for the leaſt deviations in the expreſſions of capacities, produce very conſiderable errors in the calculated degrees of heat. Therefore, however deviating the above-mentioned computed ſenſible heats are from the obſerved ones, ſtill I look upon them as very nearly anſwering the expectation. Then very ſmall corrections of the capacities may make them perfectly agree with the obſervations.

Having found that the doctrine of capacities ſo well agreed with the phenomena of cold produced by ſolutions of common ſalt in water, I was deſirous to know how far the cold, produced by mixing ſalt and ſnow, could be accounted for, from the ſame principles.

As it is well known, that when cold ariſes in a ſaline mixture, this, from a ſtate of dryneſs, grows humid, or is entirely converted into a liquid ſolution; I had no doubt but that this converſion into a liquid ſtate, and the increaſe of capacity ariſing from it, was the cauſe of the cold produced. I was confirmed in this opinion by the following obſervations. In every mixture of ſalt and ſnow, there is a maximum of producible cold, or there is a certain degree determinable on the thermometric ſcale, beyond which no farther cold, by a given mixture of ſalt and ſnow, can be produced. If the ſalt as well as the ſnow, before the mixture, are cooled below that point, there is no cold produced, and the particles of the mixed bodies continue unaltered in their dry ſtate. If the ſame mixture, by means of heat, is converted into a liquid ſolution, and afterwards expoſed to a degree of cold below that point, it will looſe its liquidity, and be converted into ſolid ice. From thence it is evident, that the loweſt point of cold, producible by a ſaline mixture, is the freezing point of that mixture; and as, at that point, no liquefaction can take place, the cauſe of the production of cold is removed. So I found that common ſalt and ſnow mixed together produced a cold of — 21°; (or — 5°,8 according to Farhenheit's thermometer) that a ſaturated ſolution of this ſalt and water, when expoſed to the free air in a lower temperature than — 21°, was partly frozen, and made a thermometer, immerſed in it, riſe to — 21°, and that the ſalt and ſnow mixed in any temperature below that degree, continued unaltered, as to their external appearance, and produced no cold at all.

Although theſe general facts perfectly agreed with the theory, yet I did not look upon them as fully convincing, till I, from more determined phenomena, was able to make computations for the cold produced, as well as for the variation of the capacities. Accordingly I mixed 1,75 parts of ſnow, and 0,625 parts of common ſalt, their common temperature before the mixture being —7°,5, in a thin globe of glaſs, whoſe capacity was 0,35. This globe was immediately put into a veſſel filled with 34 parts of water, (where to avoid prolixity I have included the capacity of the veſſel) heated to a temperature of + 21°. When the ſaline mixture was melted, and after it had ceaſed to communicate any farther cold to the water, the common temperature was obſerved to be + 15°,25. Now the capacity of the water being 1, that of the ſnow 0,9, that of the common ſalt 0,226, and that of the ſolution (as it contained 2,8 parts of water to 1 part of ſalt) 0,8; and the loweſt point of the thermometric ſcale being ſuppoſed to be ſituated at —800°, I had the whole quantity of heat contained in the materials before the mixture, equal to (1,75

$\times 0{,}9 + 0{,}625 \times 0{,}226 + 0{,}35) \times (800 - 7{,}5) + 34 \times 821 = 29551^\circ{,}5$; but the ſum of the capacities, after the mixture, are equal to $34 + 2{,}375 \times 0{,}8 + 0{,}35 = 36{,}25$; conſequently the common temperature after the mixture ought to have been $= \frac{29551{,}5}{36{,}25} = 815^\circ{,}21$, or 15°,21 above the freezing point, which does not perceptibly deviate from the obſerved temperature of 15°,25.

The reader will perceive that the loweſt degree of heat, as determined by theſe experiments of Mr. Gadolin's, is only 1400 below the beginning of Farhenheit's ſcale. From the experiments with pure and inflammable air recited above, the point of total privation appeared to be nearly 1500 degrees below the zero of Farhenheit.

It is proper, however, to remark that the capacity of a mixture of oil of vitriol and water, is ſomewhat increaſed by an augmentation of temperature; and if allowance be made for this circumſtance, in Mr. Gadolin's experiments, the reſults will nearly coincide with thoſe of the experiments on the combuſtion of pure and inflammable air.

That the capacity of a mixture of oil of vitriol and water is increaſed by an augmentation of temperature, is proved as follows.

1. More ſenſible heat is produced by mixing oil of vitriol and water, or ſtrong and weak ſpirit of vitriol at a low temperature, than by mixing them at a high temperature. Thus I have found that when ſtrong and weak ſpirit of vitriol were mixed ſucceſſively at the temperatures of 50, of 110, and of 212, the quantities of ſenſible heat produced were uniformly diminiſhed, in proportion as the temperatures were increaſed. Strong and weak ſpirit of vitriol are better adapted for theſe experiments, than oil of vitriol and water, becauſe when the latter fluids are mixed, a conſiderable evaporation is produced.

Mr. Cavendiſh was, I believe, the firſt who diſcovered that ſpirit of wine and water excite leſs ſenſible heat, when mixed at a high, than at a low temperature. I have found that the ſame thing happens when ſpirit of wine and oil of vitriol, and likewiſe when cauſtic alkali and water, are mixed at different temperatures. Theſe facts render it probable, that the compounds, which are produced by the chemical union of thoſe ſubſtances with each other, have their capacities increaſed by augmenting their ſenſible heats; for if, in ſuch experiments, the capacity of the compound be increaſed by augmenting its ſenſible heat, it will be leſs heated by the application of a given quantity of fire, at a high, than at a low temperature.

2. When ſubſtances which produce heat by chemical action are combined together, the denſity of the compound is greater than the mean denſity of the ingredients. This has been called by Mr. Kirwan the accrued denſity. Now it appears by experiment that the accrued denſity of ſpirit of wine and water, and of oil of vitriol and water, is greater when they are mixed at a low than at a high temperature. The truth of this concluſion, at leaſt as far as relates to ſpirit of wine and water, is clearly proved by the experiments of Mr. De Luc. From thoſe experiments it appears, that the expanſions of a mixture of water and alcohol, have an increaſing progreſs compared with the mean expanſions of theſe fluids in their ſeparate ſtate; and conſequently the accrued denſity muſt diminiſh, or the denſity of the compound

pound muſt approach more nearly to the mean denſity of the ingredients, in proportion as the temperature is increaſed.

I have alſo found, that when oil of vitriol and water are mixed at a high temperature, the increaſe in the denſity of the compound is leſs than when they are mixed at a low temperature.

Since, therefore, the accrued denſity of weak ſpirit of vitriol, as well as that of a mixture of alcohol and water, is diminiſhed by an augmentation of temperature; and ſince the accrued denſity, and the diminution of capacity, are effects which proceed from the ſame cauſe, that is from chemical action, there is reaſon to believe that thoſe effects will, in ſome meaſure, correſpond with each other; and conſequently, in proportion as there is leſs accrued denſity, there will be a leſs diminution of capacity. If this be admitted, it will follow that the compounds, which are produced by the chemical union of alcohol and water, and by that of ſpirit of vitriol and water, have their capacities ſomewhat increaſed by an augmentation of temperature.

3. That weak ſpirit of vitriol has its capacity increaſed by augmenting its ſenſible heat, is confirmed by direct experiment; for when equal parts of diluted ſpirit of vitriol are mixed together at different temperatures, the temperature of the mixture is greater than the arithmetical mean; and conſequently, the capacity of this fluid muſt be increaſed by augmenting its temperature, as appears from Propoſition II. page 59.

If allowance be made for this circumſtance, the reſults of Mr. Gadolin's Experiments will be found nearly to coincide with thoſe which have been deduced from the experiments on the combuſtion of pure and inflammable air. There is, therefore, the utmoſt reaſon to believe that the loweſt degree of heat, or the point of total privation, as calculated from trials upon different bodies, according to the rule propoſed by Dr. Irvine, is conſtant; and it is probable that this point is nearly 1500° below the beginning of Farhenheit's ſcale.

It is proper, however, to obſerve, that we cannot as yet be aſſured that the loweſt degree of heat is determined with perfect preciſion; for though, from ſimilar trials which

I have

I have myſelf made, I am convinced that Mr. Gadolin's experiments are nearly accurate; and though theſe experiments do not materially differ in their reſults from the trials with pure and inflammable air related in the former part of this work; yet, in order to arrive at a ſufficient degree of preciſion in a ſubject of ſuch delicacy, it is neceſſary that we ſhould take the mean reſult of an extenſive ſeries of experiments made with a view to determine the changes of capacity which ariſe from alterations of form in a *variety* of different bodies, together with the quantities of heat or cold produced by ſuch alterations.

As the method of inveſtigating the beginning of the ſcale of heat was originally propoſed by Dr. Irvine, I was unwilling to interfere with him in the ſolution of this curious problem. I have, therefore, hitherto avoided entering into this ſubject more minutely than was neceſſary for the illuſtration of the principles contained in the foregoing pages. But as that Gentleman is lately deceaſed, and as the queſtion is not yet determined in a manner that is altogether deciſive, I mean in future, if it pleaſe God to grant me life and health, to make it the object of my particular attention.

I have endeavoured, in the courſe of the foregoing work, to mark with as much fidelity and accuracy as poſſible, the improvements which were made by Dr. Black and Dr. Irvine, in the doctrine of heat, before I began to pay attention to this ſubject. The following is, I believe, a correct ſtatement of the progreſs of theſe improvements, as far as they relate to the inveſtigation of the comparative heats of bodies.

Dr. Black was the firſt who concluded from Farhenheit's experiment, recited in page 86, that mercury and water have different capacities for heat. In purſuance of this idea he made experiments to determine the comparative heats of various ſubſtances. It is, therefore, to that Philoſopher we are indebted for our knowledge of the general fact, that the capacities of bodies for heat are different.

Dr. Irvine inferred from this fact, compared with the diſcovery of latent heat, which had previouſly been eſtabliſhed by Dr. Black, that the capacities of fluid bodies are greater than thoſe of the ſame ſubſtances when they are changed into ſolids. This inference, which he afterwards confirmed by experiment, led him to the diſcovery of the rule for aſcertaining the loweſt degree of heat; and upon the ſame principles he explained the cauſe of the heat which is produced by chemical mixture.

It has been inſinuated that I publiſhed, in a former edition of this work, a part of the diſcoveries made by Dr. Irvine, without acknowledging the author. Thefollowing letter, which I received from that Philoſopher, in the year 1780, will prove the falſhood of ſuch inſinuations.

"SIR,

"As from ſome cauſe or another, probably from the miſtakes and miſapprehenſions of people ill-informed in the ſubject of our reſearches, I have been repreſented as the author of the doctrines concerning heat in your late publication; I take this opportunity of ſignifying, that I lay no claim to the diſcovery that atmoſpherical air contains *more* abſolute heat than fixed air, upon which the doctrines of animal heat and inflammation, explained in your book, ſo immediately depend. I likewiſe lay no claim to the *general fact* concerning the increaſe or diminution of the abſolute heat of bodies, in conſequence of the ſeparation or addition of phlogiſton, which is contained in your book; and in general there is nothing in your publication on Animal Heat, and the Inflammation of Combuſtible Bodies, that I lay any claim to, which you have not in that work already aſcribed to me.

Glaſgow College, "I am,
Jan. 27, 1780. "Your humble ſervant,

(Signed) W. IRVINE."

The Propoſition enunciated in page 59, is ſtrictly deduced from the definition which has been given of a permanent capacity; ſee p. 53. In that definition are included all thoſe caſes, in which equal variations of temperature are produced by the addition or ſeparation of equal quantities of abſolute heat. If, however, a permanent capacity ſhould be differently defined; if we ſhould reſtrict this expreſſion to thoſe caſes only in which no heat is abſorbed or evolved by a variation of temperature, or in which the ordinates, repreſenting the capacities in different parts of the ſcale, are equal to each other, the above-mentioned Propoſition would not be univerſally true; for we can readily ſuppoſe that when heat is communicated to a body, a part of it may be abſorbed, and a part may become ſenſible; and the proportion between the part which is abſorbed, and that which becomes ſenſible, may be conceived to be ſo adjuſted, in different temperatures, that an equal effect ſhall be produced, or that the ſame quantity of abſolute heat, which raiſes the body one or two degrees, at a given temperature, ſhall raiſe it an equal number of degrees at all other temperatures.

In this caſe the capacity would be permanent, according to the firſt definition which I have given of that quality in bodies; but, by the ſecond definition, it would be an increaſing capacity.

It is plain, however, from the reſults of the foregoing experiments on the loweſt degree of heat, that if, in particular inſtances, the capacities vary according to the law which has been now ſtated, the effects produced are nearly the ſame as if no ſuch variation took place.

I have obſerved in page 33, that if, in the experiments with warm and cold water, the mercurial thermometer had indicated the exact arithmetical mean, we could not from thence infer, with certainty, that the dilatations of this inſtrument are proportional to the increments of heat; for that if unequal augmentations of capacity were produced in water by equal increments of heat, and if unequal contractions were produced in mercury by equal diminutions of heat, theſe contrary irregularities might be conceived to be ſo adjuſted, as preciſely to balance each other. It is proper to obſerve that, in this caſe, the mercurial thermometer would be an accurate meaſure of the quantities of heat applied to water, but would not be an accurate meaſure of the quantities applied to itſelf.

In the calculations of the experiments which were made by mixing warm and cold water together, (ſee p. 20) the capacity of the veſſel was compared with that of the cold water, whereas it ſhould have been compared with that of the mixture. If allowance be made for this error, it will appear that the deviation of the mercurial thermometer from the arithmetical mean, in thoſe experiments, was a little leſs than I ſuppoſed it to have been.

Thus it has been ſuppoſed that the deviation, in Experiment III. p. 21, was .55. When the above-mentioned miſtake is corrected, it will be found to have been only .25 of a degree.

I am indeed convinced, by a careful repetition of my experiments, that, in ſuch trials, the temperature of the mixture, as indicated by the mercurial thermometer, corrected according to Mr. Cavendiſh's method, correſponds very nearly with the arithmetical mean.

It is obſerved, in page 294, that heavy inflammable air conſiſts partly of light inflammable air, and partly either of fixed air, or of a ſubſtance that may be conſidered as its baſis. This concluſion was drawn from Dr. Prieſtley's experiments, which prove that a large quantity of fixed air is produced by the combuſtion of dephlogiſticated and heavy inflammable air; but that the weight of the fixed air, which is the reſult of that proceſs, is not altogether equal to the ſum of the two airs uſed in the experiment.

The reader will perceive that I had reaſon to be convinced, from ſubſequent trials, that when heavy inflammable air is in its pureſt ſtate, it conſiſts entirely of a ſubſtance which forms the baſis of fixed air *.

As it may perhaps be uſeful to the reader to have a general view of the reſults of the experiments recited in the foregoing pages, have comprized them in the following table.

Table of the comparative heats of different bodies, as determined by the preceding experiments.

Inflammable air	21,4000
Dephlogiſticated air	4.7490
Atmoſpherical air	1.7900
Aqueous vapour	1.5500
Fixed air	1.0454
Arterial blood	1.0300
Water	1.0000
Freſh milk of a cow	.9999
Venous blood	.8928
Phlogiſticated air	.7936
Hide of an ox, with the hair	.7870

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* See page 344.

Lungs of a ſheep	.7690
Lean of the beef of an ox	.7400
Alcohol	.6021
Rice	.5060
Horſe beans	.5020
Spermaceti oil	.5000
Duſt of the pine tree	.5000
Peas	.4920
Wheat	.4770
Barley	.4210
Oats	.4160
Vitriolic acid	.4290
Pit-coal	.2777
Charcoal	.2631
Chalk	.2564
Ruſt of iron	.2500
Waſhed diaphoretic antimony	.2272
Calx of copper nearly freed from air	.2272
Quicklime	.2229
Cinders	.1923
Aſhes of cinders	.1855
Ruſt of iron nearly freed from air	.1666
Waſhed diaphoretic antimony nearly freed from air	.1666
Aſhes of the elm tree	.1402
Calx of zinc nearly freed from air	.1369
Iron	.1269

Braſs	.1123
Copper	.1111
White calx of tin nearly freed from air	.0990
Regulus of zinc	.0943
Aſhes of charcoal	.0909
Tin	.0704
Yellow calx of lead nearly freed from air	.0680
Regulus of antimony	.0645
Lead	.0352

FINIS.

Plate I.

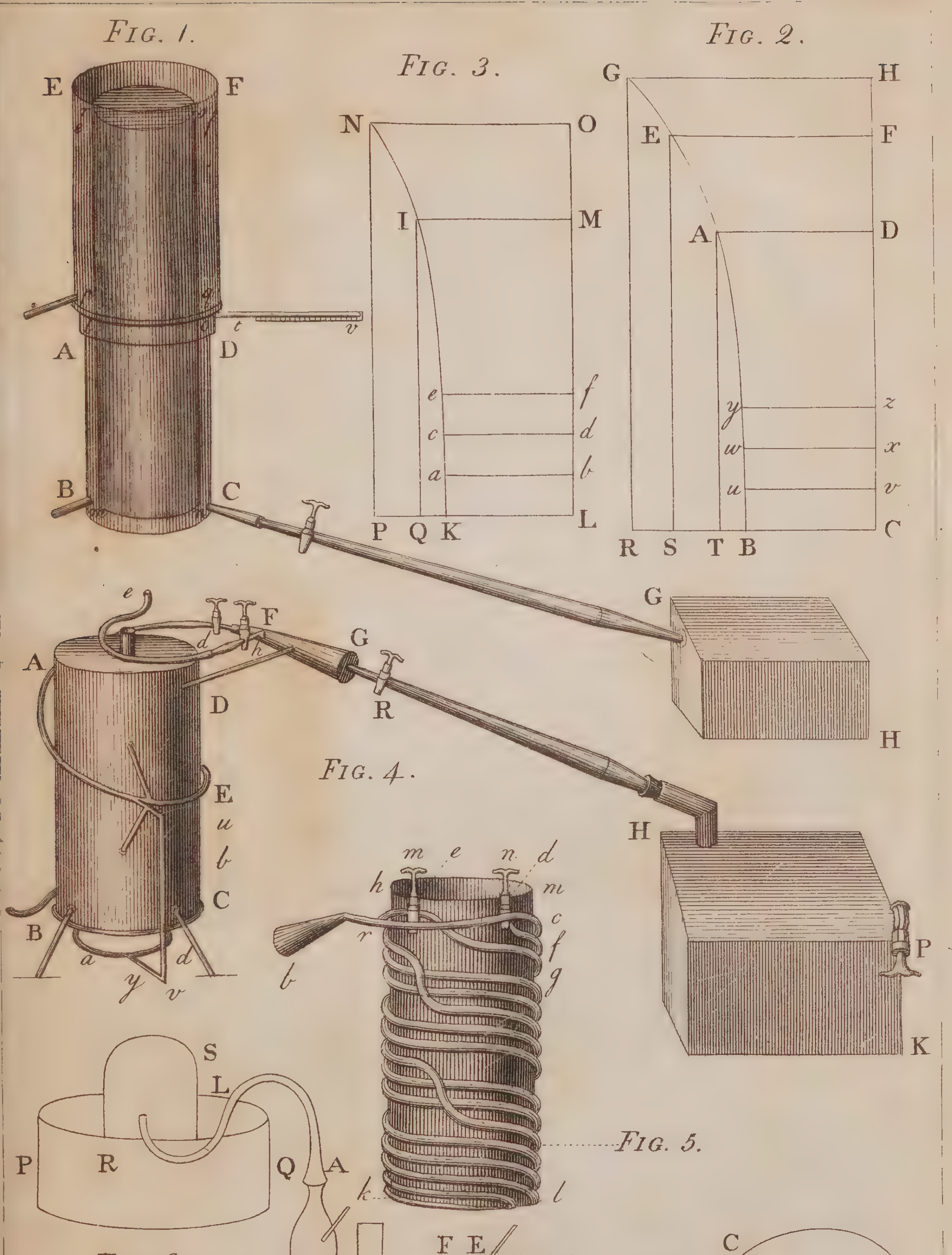

Fig. 1. *Fig. 2.* *Fig. 3.* *Fig. 4.* *Fig. 5.* *Fig. 6.* *Fig. 7.*

E F A D B C t v
N O I M e f c d a b P Q K L
G H E F A D y z w x u v R S T B C
G H
e d F h G A D R E u b C B a y v d
H P K
m e n d h m r c f g b k l
S L P R Q A
F E B M H K G C D
p l m k g h a b d c n o

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Plate II.

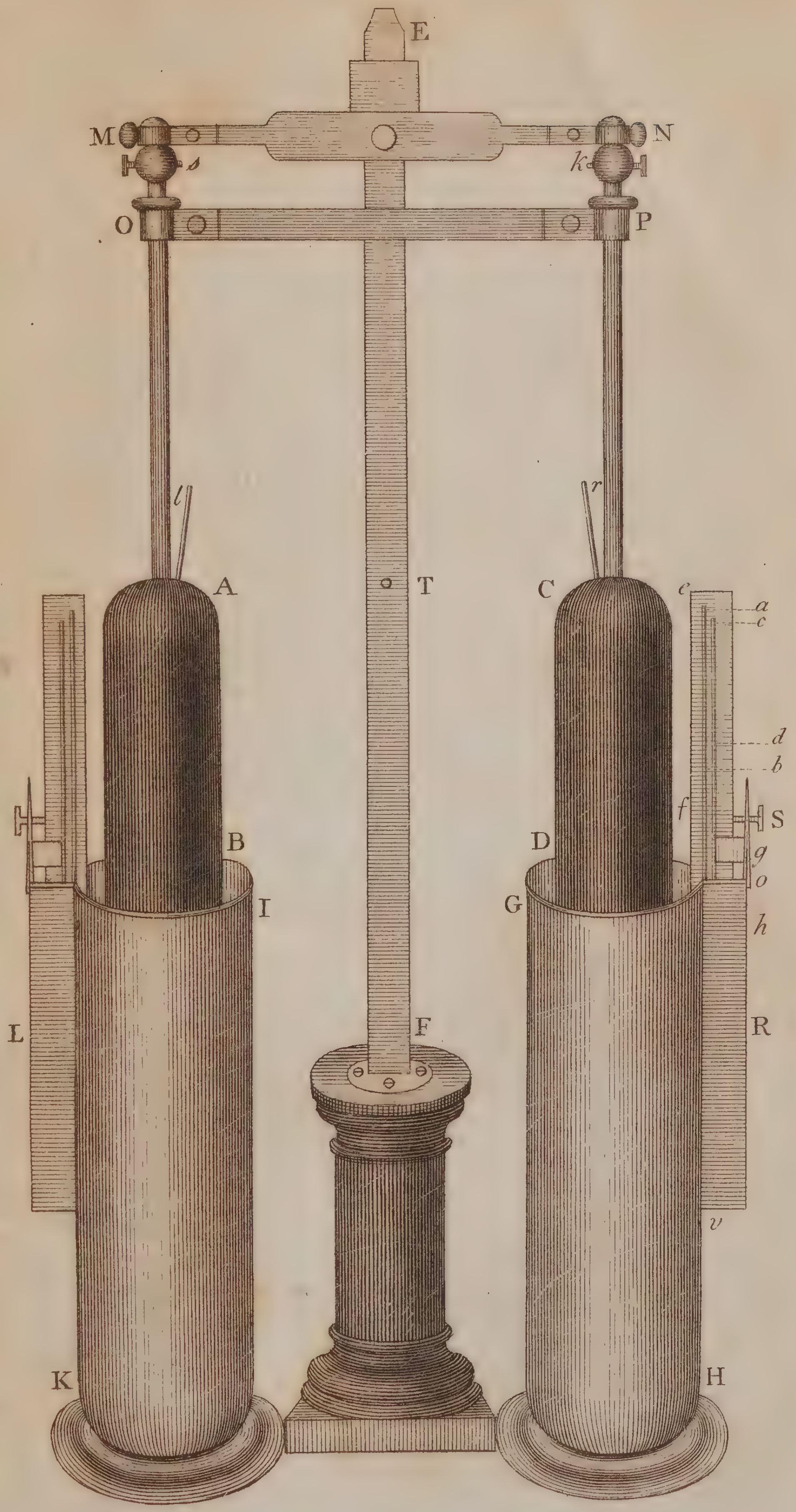

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Plate III.

FIG. 1.

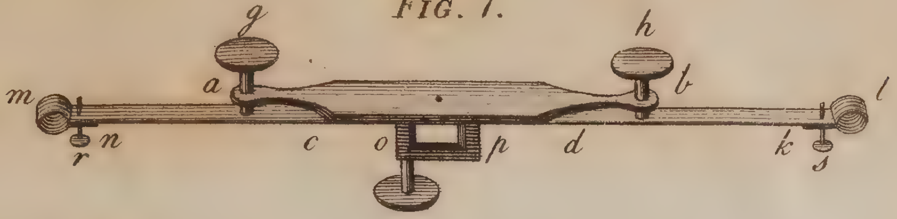

FIG. 2.

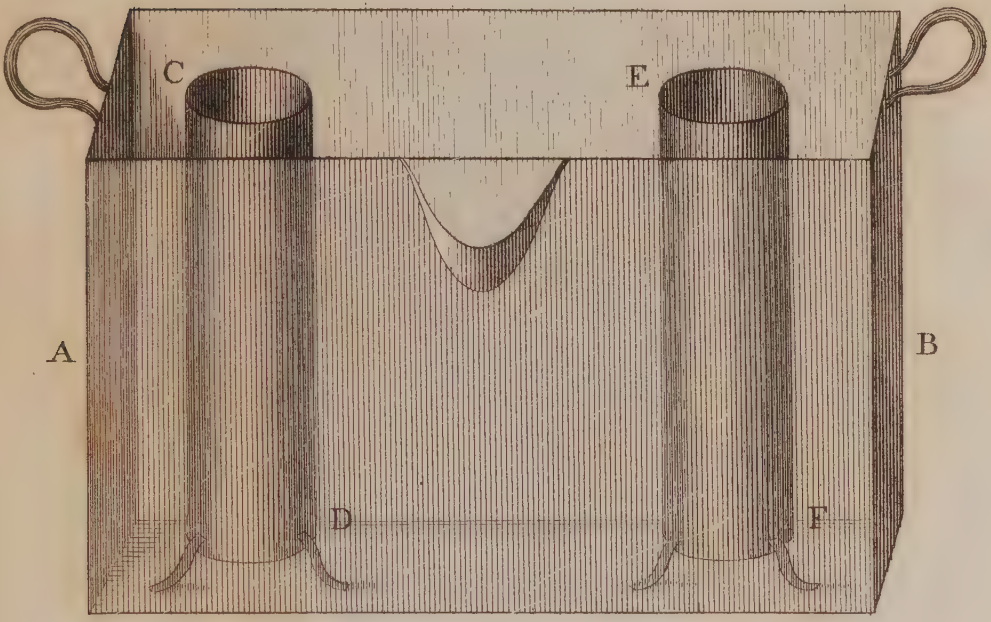

FIG. 3.

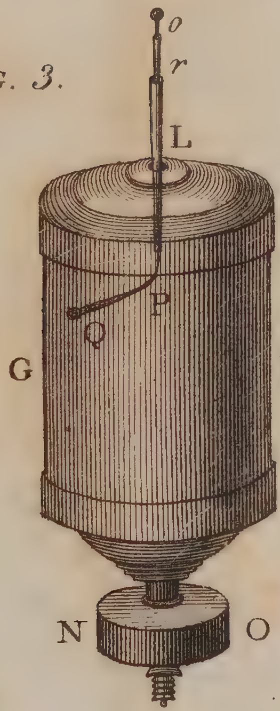

FIG. 4.

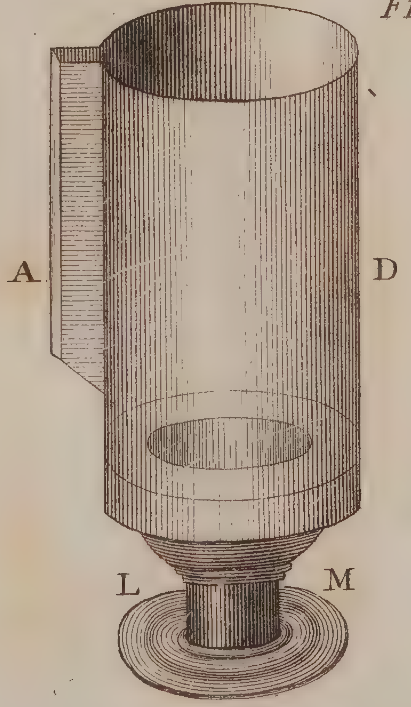

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Plate IV.

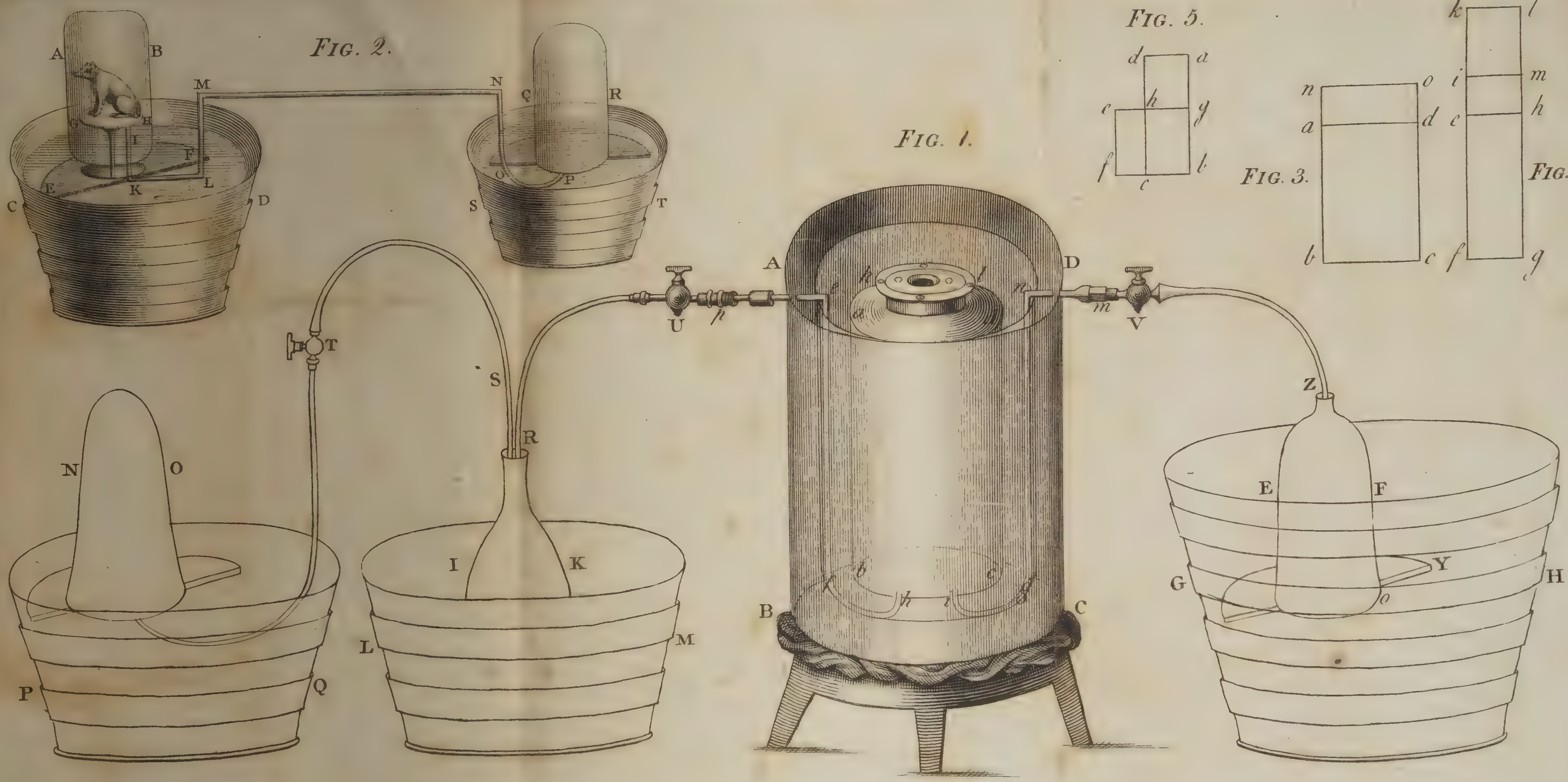

FIG. 1.

FIG. 2.

FIG. 3.

FIG. 4.

FIG. 5.

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